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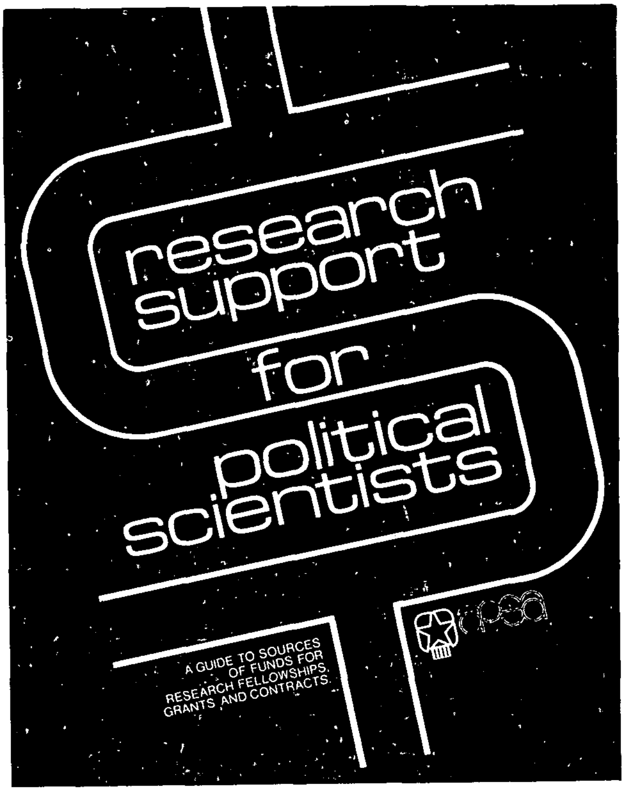
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Whites' Opposition to "Busing": Self-Interest or Symbolic Politics?*

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This article contrasts the "self-interest" and "symbolic politics" explanations for the formation of mass policy preferences and voting behavior. Self-interested attitudes are defined as those supporting policies that would maximize benefits and minimize costs to the individual's private material well-being. The "symbolic politics" model emphasizes pressures to make adulthood attitudes consistent with the residues of preadult socialization. We compare the two models in terms of their ability to account for whites' opposition to busing school children for racial integration of the public schools, and the role of the busing issue in presidential voting decisions, using the 1972 Center for Political Studies election study. Regression analysis shows strong effects of symbolic attitudes (racial intolerance and political conservatism) on opposition to busing, and of the busing issue on presidential voting decisions. Self-interest (e.g., having children susceptible to busing) had no significant effect upon either. It is concluded that self-interest is often overestimated as a determinant of public opinion and voting behavior because it is too rarely directly assessed empirically.

It has long been taken for granted that self-interest is an important, indeed even the major, determinant of political attitudes and voting behavior. Theorists from Adam Smith to Anthony Downs (1957) have assumed that citizens adopt policy preferences that further their private interests and vote for political candidates who support such policy stands. Rational choice theorists analyze citizens' preferences on political issues as a function of personal utility (see Page, 1977). And many public opinion researchers (e.g., Campbell et al., 1960; Lipset, 1960) have thought self-interest a major determinant of attitudes on domestic policy, conceptualization of policy differences, political extremism, and the political consequences of identification with reference groups such as social class, labor unions, and the political parties. These views all presuppose a common psychological theory about the formation of individual attitudes. They assume that people adopt political orientations in large part on the basis of the relative utilitarian costs or benefits of the various available attitude positions. The most pertinent theories regarding attitudes, the functional theories (e.g., Katz, 1960; Smith, Bruner, and White, 1956), would describe this as the "instrumental" or "utilitarian" function of an attitude.

As used in ordinary discourse, and by students of public opinion, a "self-interested" attitude is usually defined fairly restrictively as one which is directed toward maximizing gains or minimizing losses to the individual's tangible private well-being. Primary among these costs and benefits, of course, are specifically economic ones. But other tangible benefits are usually included as well, such as shelter, safety, and the protection of one's own and one's children's future well-being. It is of course possible to apply the term "self-interested" to attitudes maximizing every other possible form of satisfaction, as well, whether value-expressive, affiliative, moral, or whatever. But, stretched so far, the concept of self-interest becomes so all-inclusive as to be meaningless.[1] Finally, one may have formed self-interested

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[1] A parallel distinction is involved in the effort by Berelson, Lazarsfeld, and McPhee (1954, p. 184) to identify "position issues" as the primary elicitors of self-interested political behavior. Such issues raise the question "in whose *interest* should the government be run?" And "these involve tangible gains for the successful group." "Style issues," on the other hand,

attitudes because of either past experience or future prospects. One may have a good past experience (e.g., a rise in income), and then come to like things associated with it (e.g., the incumbent president). Or one may calculate the advantages or disadvantages of future policy alternatives, and support the seemingly more advantageous policy and candidates who propose it.

Empirical research designed to document the effects of self-interest upon public opinion and voting behavior has generally used one of three paradigms. Macrolevel studies have correlated aggregate statistics about costly or beneficial past events (e.g., fluctuations in the national economy) with aggregate-level statistics about public opinion (e.g., net vote for the congressional candidates of the president's party). Such studies have found statistically significant effects, though their magnitude is often debatable and they tend to occur only under rather restricted conditions (Arcelus and Meltzer, 1975; Bloom and Price, 1975; Tufte, 1978). But they are not useful for determining the extent to which individuals vote because of their own economic self-interest. They cannot determine whether or not those individuals who are being most helped or harmed by economic change are the same ones whose votes are being swayed in the appropriate direction.

Those who use a second paradigm infer self-interested attitude formation from differences between groups in attitudes that seem to differentiate those groups' interests (e.g., Campbell et al., 1960; Lipset, 1960). A simple current example would be differences between the sexes in attitudes toward women's rights. Again, such studies are inadequate for testing the proposition that individuals form attitudes that maximize their private interests. Within any given group, individuals vary widely in their interests, and the greatest enthusiasts for the group's interests may or may not be those with the greatest personal stake in the issue. For example, the women who most vigorously promote women's rights are, by and large, not the most downtrodden, poorly educated, housewives married to men most chauvinistic about male dominance, but instead tend to be the most favored, affluent, professional women. Similarly, youths most opposed to the Vietnam War were not those most vulnerable to the draft, but were females, and those males who were safely protected in deferred draft status in elite colleges. So this group-difference paradigm also is inadequate, and often misleading, when used as evidence for inferring a self-interest basis for attitudes at the individual level.

The most unequivocal tests of self-interest come from yet a third paradigm, in which the impact of an issue on the individual's personal life is directly related to attitudes and voting behavior. These have defined self-interest sometimes in objective terms, and sometimes in subjective terms. For example, one can measure the individual's income at two different times, and then objectively calculate its rise or decline, or one can simply ask the individual for a subjective impression of any changes in his or her financial well-being. The few such studies done prior to this found surprisingly little direct effect of personal impact in several areas. The personal impact of economic fluctuations seems to have had little effect upon citizens' voting behavior in congressional elections in the postwar period (Fiorina, 1978; Kinder and Kiewiet, 1979), though the 1958 recession was something of an exception (Campbell et al., 1960, pp. 390–91; Weatherford, 1978). In another case, racially based threats to whites' personal lives, such as personal economic rivalry with blacks, fear of crime in one's own neighborhood, or the prospect of neighborhood desegregation or busing, did not affect either racial prejudice or voting against a black mayoral candidate (Sears and Kinder, 1971). Similarly, in the 1974 energy crisis, citizens' preferences for government policies that would reduce energy consumption or increase energy development were unrelated to the degree of inconvenience caused by the crisis in their personal lives (Sears, Tyler, Citrin and Kinder, 1978). And Reiss (1972) observes that personal victimization by crime has not generally had much impact upon one's own attitudes toward law enforcement and the criminal justice system. These negative results leave some question about the power of self-interest in the formulation of mass political attitudes, and in individual voting behavior.[2]

A "symbolic politics" analysis leads to quite different predictions about attitude formation. According to this theory, people acquire in

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involve "self-expression of a rather indirect, projective kind," with "indirect, subjective, symbolic gratifications." These writers recognize the diversity of human motives, but to them the concept of *self-interest* means, as it does to most, direct, material, tangible gains.

[2] A number of other studies relevant to this point have been conducted since this one was completed, and will be discussed in the concluding section of this article.

early life standing predispositions which influence their adult perceptions and attitudes. In adulthood, then, they respond in a highly affective way to symbols which resemble the attitude objects to which similar emotional responses were conditioned or associated in earlier life. Whether or not the issue has some tangible consequence for the adult voter's personal life is irrelevant. One's relevant personal "stake" in the issue is an emotional, symbolic one; it triggers long-held, habitual responses. So, for example, the importance of political symbols such as "integration," "blacks," or "Harlem" is that they evoke underlying predispositions, such as the person's racial tolerance or prejudice.[3]

The "symbolic politics" model is buttressed by three basic lines of research. Work on political socialization has demonstrated that many significant social and political attitudes are first acquired in the pre-adult years. The persistence of these early attitudes into adulthood has necessarily been harder to document, but many such attitudes at least show impressive continuity with those acquired prior to adolescence, and in addition are strikingly resistant to change in adulthood (Klapper, 1960; Sears, 1975). Moreover, responses to political events in adulthood are determined to a large degree by attitudes derived from socialization in earlier life. Many studies document the power of party identification, racial prejudice, and other such residues of socialization over responses to the flow of information reaching adults (e.g., Becker and Heaton, 1967; Campbell et al., 1960; Sears and Chaffee, 1979). Similar findings have emerged from some studies intending (but failing) to document self-interest effects (e.g., Sears and Kinder, 1971). Finally, the cognitive consistency theories have inspired much empirical research documenting widespread pressures toward consistency of attitudes (Abelson et al., 1968). That attitudes toward current events would emerge as consistent with predispositions would follow from this line of work.

Whites' Opposition to Busing

The purpose of this article is to compare these two viewpoints regarding the determinants of whites' attitudes about busing of school children for racial integration of the public schools. Busing, along with affirmative action, seems to have become one of the two major current flashpoints of conflict over racial policy in the United States. In many communities in which busing has become an issue, it has inflamed public opinion and dominated local elections. It has become a national issue of some importance, and has played a role in voting behavior at the national level, though so far one not specified very clearly. As a legal issue, busing, along with affirmative action programs, is in some senses the natural legatee of the civil rights struggles of the 1950s and 1960s. As such it has caused a recurrent series of collisions among the courts, legislative bodies, executive agencies, and public opinion.

The mass white public has been strongly opposed to busing ever since it has become a visible public issue. National surveys done during the 1970s have typically found only about 15 percent of the white public supporting busing as a solution to the problem of desegregating the schools.[4] But it is not immediately clear why whites are so opposed to busing. There seems to be widespread acceptance of the general, abstract goal of desegregated schools. For instance, national surveys in 1972 and 1973 found 65 and 75 percent, respectively, supporting integration (while only 21 and 4 percent supported busing in these same surveys; see Wall, 1973; Gallup Index, 1973). The scholarly debate about busing tends to focus on whether or not it achieves the intended gains for black students, such as better school achievement or more harmonious racial attitudes, as opposed to lowered achievement for white children and/or the possibility of "white flight" and consequent school resegregation (e.g., Armor, 1972; Pettigrew, 1973). These academic arguments do not seem to tap into the emotional intensity of the most potent anti-busing arguments, however. These seemingly center on direct threats to whites' private lives; e.g., lower school quality, the inconvenience of having one's children not attend neighborhood schools, physical dangers and disruptions in schools, loss of freedom (hence "forced busing"), disrupted social relationships among children, or the added expense of busing and hence higher taxes. One might expect, therefore, a strong self-interest contribution to anti-busing sentiment.

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[3] This view overlaps in part with, and differs in part from, Edelman's provocative theorizing (1971). We share with him an emphasis on the public's attitudinal reactions to symbols. Yet Edelman gives much importance to what we have described as self-interest. He suggests that people accept political beliefs because they promise security from future threats, and because they meet people's needs for status. So a myth will be accepted to the extent that it satisfies personal interests. Our version of the "symbolic politics" hypothesis suggests instead that myths are accepted to the extent that they are congruent with long-standing attitudinal predispositions, whose preadult origins are quite irrelevant to one's adult material well-being.

[4] The exact level of support depends a little on the exact wording of the item, with greater support emerging in items with socially desirable descriptions for racial integration (e.g., "busing to achieve better racial balance"), and less in those pitting busing against attractive alternatives (e.g., "keep children in neighborhood schools" or "forget integration and provide quality education"). Gallup obtained less than 20 percent support in 1970, and 28 percent in 1974 with the former wording, while the Center for Political Studies and Watts and Free (1973) found 7 percent support in the 1972–1974 period using the latter wordings. Perhaps the modal wording was used by Gallup and NORC: "In general, do you favor or oppose the busing of Negro and White school children from one school district to another?" In five surveys from 1970 through 1974, support varied from 12 percent to 16 percent. The Opinion Research Corporation got 17 percent support in 1972, with wording unspecified (Wall, 1973). In short, the overwhelming majority of whites oppose busing, no matter how they are asked about it.

On the face of it, direct self-interest would seem to be too uncommon to be much of a force in anti-busing attitudes. About half the public school children (51.5 percent in 1973–74) in the U.S.A. ride school buses to school because of convenience, while fewer than 4 percent of all American school children were bused for purposes of school desegregation.[5] Survey research has not tested the contribution of self-interest in any very pointed way. Kelley (1974) and Weidman (1975), using national NORC data, found that having school-age children did not affect busing attitudes, but they had no measures to determine which families might be directly affected by busing. Gatlin, Giles, and Cataldo (1978), in a survey of Florida parents, found only slight effects on anti-busing attitudes of having a child who rode a bus to school, and none for racial composition of the school or for greater distances of the bus rides. But Gatlin et al. do not consider nonparents, and so are limited to testing self-interest over a rather narrow range. So there is, at the moment, little evidence on the effects of self-interest on attitudes toward busing.

The "symbolic politics" explanation also invokes powerful, deep-seated causes. A person who acquires anti-black racial attitudes early in life may respond in a similarly prejudiced manner to racially-related symbols later in life, when they surface as an integral part of some political issue. The manifest content of the political debate may not focus centrally on race, but as long as racially related symbols permeate the dialogue, and the fantasies of the citizenry, they will tend to evoke long-held racial prejudices. So, the "symbolic politics" model would suggest that racial prejudice (and perhaps opposition to social reform in general) motivates anti-busing sentiment.

To date, the evidence on this point, too, is unsatisfactory. It is clear that whites are much more opposed to "busing" than are blacks, who are usually about evenly divided about it (Kelley, 1974; Weidman, 1975). But no clear evidence exists on the direct relationship between racism and opposition to busing. Kelley (1974) and Weidman (1975) reported only a modest correlation in 1972 and 1974 NORC data, but as Erbe (1977) subsequently pointed out, the skewness of the prejudice and busing items in opposite directions set a low potential ceiling on such a correlation. Gatlin, Giles, and Cataldo (1978) find a higher correlation in their sample of Florida parents. Prejudice was nonsignificant in their complete regression model, but this null finding appears to be misleading. They included general support for school integration as a predictor in the model, and in all likelihood this variable not only carried much of the effect of prejudice, but was also itself influenced by the ostensible dependent variable (attitudes toward the local desegregation plan). This renders the model indeterminate for our purposes. Hence what is needed is an analysis that both deals satisfactorily with the skewness problem, and utilizes measures of prejudice that have nothing to do with the schools.

The major goal of our empirical analysis will be to compare the self-interest and symbolic politics explanations for whites' opposition to busing. Whites' self-interest in the busing issue presumably stems primarily from fears regarding the vulnerability of their own children to busing for racial integration, and, more peripherally, from its possible impact upon the neighborhood, such as reducing property values, raising tax rates, and so on. Such perceived threats to parental and neighborhood interests could be evoked by current busing, or by the threat of future busing. The "symbolic politics" approach views anti-busing sentiment as primarily derived from long-standing political predispositions, specifically racial prejudice and political conservatism, which in turn can be traced to political socialization in preadult life.

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[5] These estimates are from an HEW report (*Los Angeles Times*, August 10, 1976, p. IV-3) and a U.S. Civil Rights Commission Report (*New York Times*, August 25, 1976, p. 21).

Hence anti-busing sentiment should be more closely related to racial prejudice and political conservatism than to self-interest, and these in turn should be traceable to socialization in early life.

A second question is whether the political impact of the busing issue is best accounted for by a self-interest or a symbolic politics explanation. As a general matter, the question is whether issue-based voting increases when the voter has a personal stake in the issue, or whether the symbols associated with the issue affect voting behavior regardless of the voter's self-interest. In our specific case, which will focus on the 1972 presidential election, the Nixon White House strongly opposed busing as a school desegregation policy during the early 1970s (Wise, 1973), so the issue became entangled in presidential politics. Hence the self-interest hypothesis would be that anti-busing attitudes ought to contribute more to Nixon voting among the self-interested than among the disinterested. The symbolic politics hypothesis would be that they would do so irrespective of self-interest.

Whites' opposition to busing is a useful case for the contrast between the self-interest and symbolic politics models for both theoretical and practical reasons. The two approaches have quite different implications for government desegregation policy. If the self-interest explanation for opposition to busing is correct, then government needs to deal with the reality of aggrieved parents and children whose personal lives are being disrupted by school desegregation. If the symbolic politics explanation is more accurate, government is simply once again met with massive white unwillingness to make sacrifices to achieve racial equality, and rhetoric about disrupted private lives mainly represents rationalization of the anxieties associated with racial prejudice. The implication would be that the prejudice itself would have to be addressed. So we have focused on the busing issue both because it can illustrate the theoretical contrast between the self-interest and symbolic politics viewpoints, and because it is an intrinsically important policy issue.

Method

Our data base is the 1972 Center for Political Studies election study. The overall sample size was 2705, though for some analyses it is reduced to 1072, due to the use of split-sample interview forms, and attrition for the post-election interview. Since we are concerned only with white respondents, the number of cases is reduced still further, normally by about 10 percent.

Self-interest in the busing issue was indexed with three items. (1) The first question asked whether or not busing was occurring or threatened in the respondent's area: "In some places, school boards are taking some children out of their closest neighborhood schools and sending them by bus to other schools farther away. Has anything like this happened around here?" (24 percent said yes), and, "Have you heard any talk that this might happen around here in the future?" (another 13 percent said yes.) (2) Second, did he/she have any children aged 5–18, who "go to school here," and who were attending public, rather than private or parochial, school? (34 percent had one or more children in public school.) (3) The third measured the racial composition of neighborhood schools, based on an additive index composed of two items: "Is the grade school nearest you all white, mostly white, about half-and-half, mostly black, or all black?" and a parallel item for the "high school nearest you" (for 23 percent, both were "all white"). A composite self-interest scale, summing the responses to all three items, was used in some analyses.

Racial intolerance was measured with eight items dealing with areas other than school integration: support for racially restricted housing; support for segregation in general; belief in whites' intellectual superiority; opposition to federal government civil rights action promoting fair employment practices and desegregated public accommodations; and opinions regarding racial protest, such as whether it was moving too fast, was helping or hurting the blacks' cause, and was mainly violent or not. A principal components factor analysis of these items with no rotation yielded a powerful first factor reflecting generalized racial intolerance. This first factor accounted for 35 percent of the variance in individual items with each item loading approximately equally and in the same direction on it (the range of loadings being .68 to .48). The next two factors accounted for 13 percent and 11 percent of the variance. Since the first factor was the clearest, by far the strongest and the easiest to interpret, we simply created one scale of racial intolerance, adding responses to all eight items. The other predictors were standard demographic variables previously found to be associated with racial attitudes (Campbell, 1971; Middleton, 1976): age in years, years of education, and region (South vs. other regions). Finally, we also used an item measuring liberal-conservative self-identification, which had seven labeled points ranging from "extremely liberal" through "extremely conservative."

The main dependent variable was support for or opposition to busing for racial integration. The item's exact wording is as follows: "There is much discussion about the best way to deal with racial problems. Some people think achieving racial integration of schools is so important that it justifies busing children to schools out of their own neighborhood. Others think letting children go to their neighborhood schools is so important that they oppose busing. Where would you put yourself on this scale, or haven't you thought much about this?" The respondent was then given a seven-point scale, of which only the end points were labeled ("Bus to achieve integration" and "Keep children in neighborhood schools"). The percentages giving each response, from pro- to anti-busing, were 5, 2, 2, 5, 4, 7, and 75 percent (based on the responses of 2491 whites before the 1972 election). This skewness is itself quite unusual on the CPS seven-point scales, underlining the pervasiveness and intensity of anti-busing feeling. However, as indicated earlier, it is probably not specific to this particular item, since other busing items used by other organizations obtain very similar marginals. It does demand special statistical precautions, however, which will be elaborated later on.

Determinants of Opposition to Busing

These marginal frequencies already fuel our suspicion that direct self-interest is not a strong factor in whites' opposition to busing. Less than half the respondents had any self-interest on each item, while the overwhelming majority opposed busing. Still, substantial numbers did have a personal stake in the issue; e.g., 27 percent of those living in districts with ongoing busing had children in the public schools, as did 45 percent of those living in districts with busing threatened. So self-interest could play a significant role.

However, these self-interest measures yielded only weak correlations with opposition to busing; the Pearson r's were .02, .02, and −.01 for busing in the area, having a child in public schools, and racial composition of neighborhood schools, respectively. Both symbolic political attitudes were much more strongly related to anti-busing attitudes; for racial intolerance, $r = .36$, and for conservatism, $r = .30$. The strength of these latter relationships is illustrated by a simple cross-tabular analysis, as shown in Table 1.

Moreover, symbolic political attitudes completely overwhelm self-interest when included in a full regression analysis. These data are shown in Table 2, column 1. Both racial intolerance and political conservatism had strong effects, while none of the self-interest variables had an effect even approaching statistical significance. The R^2 is 19.5 percent, which reflects, in absolute terms, rather strong effects of racial intolerance and political conservatism.

The predictiveness of the model can be improved, without altering our basic conclusions, by including non-linear effects of the predictors. It turns out that only among respondents with negative intolerance scores (those giving more tolerant than intolerant responses) does tolerance/intolerance have an impact on attitude toward busing. This impact is strongly non-linear, and was found to be fitted best by a function which is the cube of the negative intolerance scale scores, and zero for positive scale scores. Table 2, column 2, shows the result of using this intolerance-cubed predictor instead of the original linear intolerance scale, and adding a term which is the square of the conservatism score, for the same reasons. This non-linear model is clearly superior, raising the R^2 from 19.5 to 29.1 percent. However, it in no way alters the major substantive finding: symbolic political attitudes predict anti-busing attitudes quite strongly, while self-interest has no significant effect.

Possible Artifacts. While these results seem so strong as to be unequivocal, the highly skewed distribution on the busing item poses the risk of underestimating correlations with other variables, especially with the self-interest items, which are somewhat skewed in the opposite direction (though not severely; they yielded splits of about 2 to 1). However, we performed a number of checks on this possibility, which have left us satisfied that the impotence of self-interest can in no way be attributed to this problem. First we looked at subgroups in which the busing item was not so badly skewed. We began by removing the most extremely prejudiced respondents from consideration. The intolerance scale had 17 points on it, but those giving intolerant responses to a majority of the items (scores of +5 and greater) were almost unanimously anti-busing. Their means on the seven-point busing scale are above 6.9, and their standard deviations about 0.3. Since in this range there is essentially no error of prediction including these cases in a regression causes us to underestimate the standard error of prediction among the somewhat tolerant segment of the population where some support for busing is found. Thus the statistically conservative strategy is to avoid problems of heteroscedasticity by deleting these most extremely intolerant respondents. Doing so (it amounts to deleting about 13 percent of the sample) does not alter the picture a bit. The R^2 shown in Table 2, column 3 remains about the same (28.7 percent), and the symbolic attitudes retain their dominance over non-significant self-interest effects.

A parallel effort was to look only at ideological liberals, another subgroup within which the busing item was not as severely skewed. Considering only self-designated "liberals" (16 percent of the sample), intolerance is still far and away the most powerful predictor of anti-busing feeling, and self-interest has no effect at all. This is shown in Table 2, column 4. In fact, our equation is more accurately predictive within this subgroup than in any other subgroup tested; the R^2 rises to 44.0 percent for "liberals." This finding has some considerable substantive interest as well, since it

Table 1. Percent of Whites Extremely Opposed to Busing as a Function of Symbolic Political Attitudes

	Political Conservatism							
Racial Intolerance	Conservative		Moderate/ Middle of Road		Liberal		Total	
High	95%	(122)	92%	(243)	85%	(27)	95%	(392)
Medium	90	(255)	85	(470)	77	(79)	86	(804)
Low	69	(153)	71	(210)	39	(198)	59	(561)
Total	85	(530)	84	(923)	53	(304)		

Source: Computed from data collected in the 1972 Center for Political Studies election study.

Note: Entry is percent at point 7 of busing scale among those at designated combination of intolerance and conservatism. The intolerance scale ranged from –8 to +8, and was cut below –2 and above +2. The 7-point conservatism scale was cut below 4 and above 4. N's given in parentheses.

Table 2. Determinants of Whites' Opposition to Busing

	All Respondents			"Liberals"	School Integration Item
	Simple (1)	Corrected (2)	(3)	(4)	(5)
Self-Interest					
Busing Happening/Heard of	–.02	–.01	–.02	.06	.09*
Child in Public School	.05	.05	.05	.03	.03
White Neighborhood Schools	.00	–.02	–.01	.09	–.03
Symbolic Attitudes					
Intolerance	.26**	–	–	–	.47**
Intolerance Cubed	–	.39**	.39**	.50**	–
Conservatism	.23**	.19**	.18**	.13	.10*
Conservatism Squared	–	.10*	.08*	–	–
Demographic Factors					
Education	–.10*	–.05	–.06	–.18*	.09*
Age	.04	.06*	.07	–.03	.00
Southern Residence	.02	.05	.04	.06	–.04
Multiple *r*	.441	.540	.536	.664	.494
R^2	19.5%	29.1%	28.7%	44.0%	24.4%
N	853	853	746	135	743
Range Restriction on Intolerance	No	No	Yes	Yes	No

Source: Computed from data collected in the 1972 Center for Political Studies election study.

Note: Entries are betas, using a standard regression analysis.

$^{*}p < .05$
$^{**}p < .001$

is often thought that liberals' integrationist sentiments are particularly vulnerable to self-interest. That is, they are often accused of turning segregationist when their own children are threatened with busing. However, in our data there is no evidence of this "fair-weather liberal" syndrome.

Another strategy was to transform the distribution of the dependent variable in the entire sample. First we used a square-root transformation on it. When the regression shown in Table 2, column 3, is redone that way, very little changes. The R^2 drops off a little (to 26.4 percent), but the substantive findings do not change. Finally we tried a dependent variable less skewed than the busing item, an old item generated in the days of school segregation in the South: "Do you think the government in Washington should see to it that white and black children go to the same schools or stay out of this area as it is none of its business?" Responses to this item were much more evenly distributed: 45 percent of those expressing an opinion favored integration. There were no nonlinear effects on it. Using it yields the same substantive findings. These are shown in Table 2, column 5. Racial prejudice is by far the most important predictor with conservatism trailing far behind. This time, however, there is one significant self-interest effect: those living in communities where busing has happened or has been proposed are somewhat more opposed to government intervention. But it is important to note that this finding does not establish any *personal* relevance of this issue; indeed, parents and nonparents react in exactly the same way.

So, if anything, the skewness in the busing item seems to have been suppressing the effects of intolerance and conservatism. There is no evidence that it is masking any true self-interest effects, when it is investigated empirically. When we look at subgroups within which anti-busing feeling is not so skewed (the more racially tolerant and/or the ideologically liberal), our basic results emerge even more strongly than ever. When we use a square-root transformation, or a non-skewed alternate school-integration item, the same results emerge. Intolerance and conservatism have strong effects, while self-interest has only negligible effects.[6]

A More Complete Model. Having dealt with these potential artifacts, and hopefully having laid them to rest, we need to resume our main argument. Our initial tests of self-interest were crude. Having children in the public schools and living in neighborhoods with all-white schools ought to create self-interest only given the current presence, or perceiving a future possibility of busing for racial integration within the respondent's own immediate, personal life space.

A final refinement of the self-interest hypothesis, then, assesses the effects of having public school children, or all-white neighborhood schools, only among people tangibly touched by or expecting busing. So our basic regression analysis (Table 2, column 3) was run separately for those living in areas where busing is happening, where the respondent has heard that it might happen, and where neither has occurred. As shown in Table 3, columns 1–3, this modification too makes very little difference. Intolerance continues to be by far the strongest predictor of anti-busing attitudes. Self-interest continues to have no effect; the critical terms are those for the child-in-public-school and white-neighborhood-schools variables, among those for busing-happening or busing-heard. None approaches significance.[7]

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[6] Another potential methodological problem stems from the greater variability in the racial intolerance scale than in the self-interest items. This alone could generate differential correlations with opposition to busing. A simple cross-tabular analysis shows that this cannot possibly be the case, though. As can be seen in Table 1, racial intolerance increased opposition to busing by 36 percent, and conservatism did so by 32 percent, when both were trichotomized. However, simple cross-tabulations of the three self-interest items (two trichotomized and one dichotomized) yielded differences in opposition to busing of only 3, 0, and 4 percent, respectively. Moreover, a composite self-interest scale, summing the responses to all three items, begins to approximate the variance of the two symbolic attitude scales, yet it correlates only .02 with opposition to busing. There seems to be no way in which differences in variance could account for these results, therefore.

[7] An even more appropriate test of this comparison is to redo the same basic regression (Table 2, column 3), creating four self-interest interaction terms (of the child-in-school and white-neighborhood-schools variables by dummy variables for busing-happening and for busing-heard-of) and dropping the main effect for the busing-happened/heard-of variable. When this is done, the R^2 is improved only slightly (to 29.4 percent), and none of the interaction terms nor the remaining self-interest main effects approach significance.

Treating the self-interest variables in other ways also made little difference. The composite self-interest index yielded the same results as the individual self-interest items. We also substituted reports of neighborhood racial composition for the item on racial

The symbolic politics model goes further into the antecedents of policy attitudes such as this one, however, by suggesting that these symbolic attitudes in turn have their origins in preadult socialization. We have attempted to index socialization with certain demographic variables; specifically, having Southern background, having been reared as part of an earlier generation, and having less than average education. And indeed they are substantially correlated with our racial intolerance measure (r = .34, .21, and .33, respectively). They are more weakly correlated with conservatism (r = .04, .17, and .12, respectively), as might be expected both because such has been the conventional finding from past research, and because it is less likely that conservatism is a product of distinctly preadult socialization (e.g., Converse, 1964). If they are products of preadult socialization, they ought not to be based on self-interest in the busing issue, and they are not; r = .02 and .01, respectively, with the self-interest composite measure. Third, these demographic measures are modestly correlated with anti-busing sentiment (r = .21, .16, and .14, respectively). But finally, the critical finding for this model is that these latter correlations disappear with symbolic attitudes controlled. Review of Tables 2 and 3 shows that none of the demographic variables has any very consistent effect upon opposition to busing when symbolic attitudes are included in the equations. Thus the impact of the demographic factors on anti-busing sentiment flows through the symbolic attitudes.

The power of symbolic attitudes, and impotence of self-interest, held across a wide variety of social groups. For example, it was plausible to hypothesize that highly educated respondents might show the strongest self-interest because they might have the most to lose from the widely expected deterioration of the quality of public education following desegregation. But as shown in Table 3, columns 4–6, they did not. Only the power of symbolic attitudes increased as the level of education increased. Indeed, intolerance was even more preeminent among the college-educated, consistently with Kelley's findings (1974). And the

Table 3. Whites' Opposition to Busing: Supplementary Analyses

	Classified by Busing in Area			Respondent's Education		
	Happening (1)	Heard of (2)	Neither (3)	High School (4)	High School (5)	College (6)
Self-Interest						
Busing Happening/Heard of	–	–	–	–.01	–.01	–.04
Child in Public School	.08	–.06	.04	.08	.14*	.00
White Neighborhood Schools	–.07	.15	–.02	–.10	–.02	.04
Symbolic Attitudes						
Intolerance	–	–	–	–	–	–
Intolerance Cubed	.36**	.49**	.36**	.21*	.13*	.49**
Conservatism	.03	.16	.26**	.14*	.14*	.20**
Conservatism Squared	.01	.00	.13**	.02	.00	.13*
Demographic Factors						
Education	–.01	–.33**	–.04	.08	.03	–.11*
Age	.16*	–.14	.04	.10	.07	.08
Southern Residence	.09	–.14	.04	.07	.07	.00
Multiple r	.480	.699	.574	.341	.268	.694
R^2	23.1%	48.8%	33.0%	11.6%	7.2%	48.2%
N	153	82	537	235	256	255
Range Restriction on Intolerance	Yes	Yes	Yes	Yes	Yes	Yes

Source: Computed from data collected in the 1972 Center for Political Studies election study.

Note: Entries are betas.

*$p < .05$
**$p < .001$

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composition of neighborhood schools. For the "busing-happening" respondents, people in all-white neighborhoods were not more anti-busing. But this measure of all-white neighborhood did yield significantly greater anti-busing sentiment for the "busing-heard-of" respondents (beta = .19, $p < .05$).

results were parallel for southern and nonsouthern respondents, despite their quite different levels of prejudice.

In short, our best test of the comparison between self-interest and symbolic politics lies in Table 3, columns 1 and 2: the results for respondents living where busing is happening, or where it might happen. Predictor variables are specified as well and fairly as they can be in this data set (and quite fairly in absolute terms, we believe), and the methodological quirks in the scales are compensated for as well as they can be. And the critical findings are, as they were in all the analyses, that self-interest was overpowered by symbolic attitudes—that is, racial intolerance and political conservatism, which appear to be the major determinants of whites' opposition to busing. This contrast was quite robust, holding up with a variety of controls on demographic variables, across different indices of opposition to school integration, and treating self-interest in a variety of different ways.

Self-Interested Issue Voting

The possible motivating force of self-interest on policy-based voting was the second major question to be addressed. The basic self-interest hypothesis was that people will use any given policy attitude in arriving at a vote decision more if they are self-interested in the policy in question than if they are not. So, busing policy should be more decisive in determining presidential votes for people who have a direct, personal stake in the issue than for disinterested people.

To test this hypothesis, we used the analytic framework developed by Miller, Miller, Brown, and Raine (1976), who have conducted the most complete analysis of 1972 presidential voting. They found voting behavior most affected by party identification and by a specific set of issues. To create a highly reliable measure of partisanship, we developed a composite measure based upon (1) the standard seven-point scale, (2) the two thermometer-feeling items toward the Democrats and Republicans, and (3) the four open-ended questions concerning what was liked and disliked about each of the parties.[8] An additive scale was then generated from these three separate measures of partisanship, and standardized for further analysis. To obtain simplified measures of the relevant issue attitudes, we conducted a factor analysis of twelve of them. With a varimax rotation, this yielded four factors, the same number but slightly different from those used by Miller et al. (1976, p. 766): questions regarding (1) the military: items on the Vietnam involvement, withdrawal from Vietnam, military spending, and amnesty; (2) marijuana/women: legalization of marijuana, equality of women, access to abortions, and laying off women with husbands before other workers; (3) minority rights: rights of the accused, and special government aid to minority groups; and (4) economic welfare: government-guaranteed jobs, and government health insurance.[9] Additive scales were formed for each of the four issue dimensions, and these were standardized before being used in further analysis.

First we attempted to replicate the Miller et al. results, using our slightly (but, we think, not consequentially) different operations. The dependent variable was pre-election preference for Nixon or McGovern.[10] This is shown in Table 4, column 1, and again indicates an overwhelming effect of partisanship, along with somewhat more modest, but still significant effects of attitudes concerning military policy, economic welfare, and busing policy. Two issue domains, marijuana/women and minority rights, had no effect and were dropped from the later analyses. Similarly, general racial prejudice had no significant effect and was not used subsequently.

The remaining analyses in Table 4 reveal no self-interest basis at all for the contribution of busing policy to the vote. The critical analysis involves the interaction of the busing policy item with self-interest in the busing issue. That is, does the effect of the busing policy item upon voting choice increase with self-interest, as the "self-interested policy-based voting" theory would suggest? Column 2 shows that these interaction terms are trivial, and do not approach statistical significance. Column 3 shows that they still have no effect even if we limit ourselves to those respondents who perceived Nixon as more opposed to busing than McGovern.

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[8] The correlations among the three were .46, .53, and .57. A factor analysis yielded one major factor accounting for 68.2 percent of the variance.

[9] The factor loadings were all .58 or greater, and no item loaded on any other factor above .30 (except the Vietnam involvement item loaded .35 on the minority rights factor). These four factors accounted for 53.4 percent of the variance in these issue items; the smallest eigenvalue was 0.96.

[10] We are well aware of the problems with error heteroscedasticity and non-normality inherent in the use of dichotomous dependent variables in multiple regression. Thus we replicated each of the regression analyses using maximum likelihood estimation of the logistic model (Nerlove and Press, 1973), which suffers from neither of these problems. Exactly the same results were obtained.

But, as we reasoned earlier, the best test of self-interest is the effect of having a child in the public schools, or of having all-white neighborhood schools, *given that* busing is already happening in the area, or that it is proposed. Hence we consider the most pointed test of self-interested policy-voting to be the interactions of the busing policy item with having a child in the public schools, or having all-white neighborhood schools, given that busing is happening (column 4) or threatened (column 5) in the respondent's area. In other words, when busing is a reality in the respondent's private life, having a child in the schools ought to fuel a busing-based presidential vote. Not having such a child ought to make respondents disinterested and make them base their votes on something else. As can be seen, these interaction terms are not statistically significant. The largest beta does not even approach statistical significance: $F = 2.38$, $p > .10$). These non-significant interactions are the best evidence, we feel, of an absence of self-interested issue voting in the case of busing in the 1972 presidential election.

Positive evidence that "busing" affected presidential voting as a symbolic issue comes from two findings. It continued to contribute to Nixon votes in most analyses independent of any self-interest term. Anti-busing people were more pro-Nixon, even with numerous other issues taken into account. And racial prejudice, which contributed marginally ($p < .10$) to Nixon voting when the busing policy item was not considered (not shown in Table 4) dropped out completely when busing was included (Table 4, column 1). Apparently "busing" was the primary issue vehicle for the influence of racial prejudice on 1972 presidential voting.

So our second main finding is that "busing" affected voting behavior as a symbolic issue, rather than as a matter of self-interest. Opposition to busing contributed significantly to votes for the more anti-busing presidential candidate in 1972, Richard Nixon. But it made no larger contribution to his vote among voters with a personal stake in the busing issue than among disinterested voters.

Table 4. Determinants of 1972 Presidential Preference

	All Respondents		Respondents Perceiving Nixon as More Anti-Busing	Respondents Classified by Busing in Area		
	(1)	(2)	(3)	Happening (4)	Heard of (5)	Neither (6)
Partisanship	.500**	.486**	.342**	.374**	.517**	.521**
Policy Attitudes						
Military/Vietnam	.211**	.214**	.314**	.301**	.194*	.189*
Drugs/Women	.024	–	–	–	–	–
Minority Rights	.003	–	–	–	–	–
Economic Welfare	.078*	.084*	.079*	.184*	.125	.030
Busing	.147**	.163**	.304**	.151	.092	.171**
Intolerance	.028	–	–	–	–	–
Busing Policy x Self-Interest Interactions						
Happened/Heard of	–	.022	.067	–	–	–
White Schools	–	.005	.059	.019	.140	.012
Child in School	–	.006	.028	.068	.079	.030
Multiple *r*	.687	.688	.804	.679	.671	.703
R^2	.472	.473	.646	.461	.450	.494
N	745	745	317	175	83	487

Source: Computed from data collected in the 1972 Center for Political Studies election study.

Note: Entries are betas.

$^{*}p < .05$
$^{**}p < .001$

Conclusions

Self-Interest. These findings are consistent with a "symbolic politics" rather than a self-interest model of the political impact of the busing policy issue. Our primary reason for doing this study was to compare these two models of attitude formation, as a general theoretical issue and quite aside from the politics of "busing." We cannot leap from this one failure to any very grand challenge to the importance of self-interest in general. But these findings do parallel the findings of numerous other studies which have assessed self-interest in terms of the direct impact of a policy issue on the individual's private life. Other national (Sears, Lau, Tyler, and Allen, 1978) and local (Gatlin, Giles, and Cataldo, 1978; Kinder and Sears, 1978; McConahay and Hawley, 1977) studies of busing have found little or no effect upon whites' policy attitudes of such self-interest variables as having a child in the public schools, current busing, length of bus ride, racial composition of host school, perceived probability of future busing of one's own child into minority neighborhoods or of minority children into white neighborhoods, or racial composition of the respondent's neighborhood. Similarly, other indicators of direct threats posed by blacks to whites' well-being have had little influence upon whites' prejudices, or voting behavior in races with black candidates (Kinder and Sears, 1978). The only apparent exception is a survey done in Los Angeles just before the announcement of a long-awaited busing plan, under conditions of great uncertainty and anxiety; parents of potentially affected white children were then somewhat more opposed to busing (Allen and Sears, 1978).

Similarly, falling personal financial fortunes seem to have had little influence upon voters' behavior in congressional elections (Kinder and Kiewiet, 1979), or upon policy attitudes regarding government-guaranteed employment; nor was this latter influenced by being personally unemployed or temporarily laid off. Not being covered, or being inadequately covered, by private medical insurance only slightly enhanced support for a nationalized health insurance plan (Sears, Lau, Tyler, and Allen, 1978). However, California's property-tax-reducing Proposition 13 was supported most strongly by homeowners, and least strongly by government employees (Sears, 1978).

Finally, the personal costs of several other policy issues seem *not* to have influenced policy attitudes about them. Having relatives or close friends in Vietnam did not affect attitudes toward that war (Lau, Brown, and Sears, 1978), the personal hardships attendant to the 1974 energy crisis did not influence policy attitudes regarding energy conservation or development (Sears, Tyler, Citrin, and Kinder, 1978), and a variety of indicators of crime victimization generally have little effect upon "law and order" policy attitudes (Reiss, 1972; Sears, Lau, Tyler, and Allen, 1978; Tyler, 1978).

In short, substantial evidence based on a variety of different policy issues has obtained disappointingly small direct effects of self-interest on the formation of policy attitudes, and on voting behavior. This research ought at least to raise the possibility that self-interest has generally been overestimated as a force in the generation of popular mass political attitudes. Our best guess is that it has been overestimated because it has so rarely been assessed directly; researchers have too often settled for the aggregate-level or group-difference paradigms. The present line of work suggests that, when self-interest is assessed directly, at the individual level, its effects may be quite meager.

Further research might lead in two quite different directions. One is to explore further the actual boundaries of the role of self-interest in attitude formation and change. It must have important effects in some situations, which might be detected with increasingly pointed research. Self-interest measures tailored more specifically to the individual's exact situation, or investigation of more proximal, local political issues, might prove more fruitful.[11]

Or research might explore the psychological process by which people relate (or, apparently, more often do not relate) their private interests to their preferences in public policy and voting. Four possibilities immediately assert themselves here. There is the familiar cognitive deficiency argument: through deficient socialization, information, or processing capabilities, most citizens may simply be unequipped to make the necessary complex calculations necessary to analyzing their own self-interest. Alternatively, people may feel (quite sensibly) that their own needs and experiences provide too narrow an informational base for decisions affecting the collective good. Instead, they resort to more general principles about what is right and wrong (which we have described as "symbolic attitudes") to guide their vote decision making when they vote. Third, perhaps the answer is to be found in political socialization. Children may generally get socialized to a norm of making voting decisions in terms of the common, public interest, rather than in terms of selfish private interests. When they find themselves in the role of citizen later in life (in the voting booth or the survey interview), they may then behave in a community-spirited, altruistic, "public-regarding" manner (Campbell, 1965; Wilson and Banfield, 1971). This is certainly an aspect of early socialization that has not been carefully researched. A final possibility is that people do not perceive their own votes to be truly instrumental to policy decisions. This could occur because of general alienation or sense of inefficacy; that is, the generalized belief that the government does not listen to individual citizens, or that individual votes do not influence the outcome. Or it could be more specific to the particular issue in question. The voter may believe that the candidates do not really differ, that the issue itself is not important enough to influence a presidential vote, or that the local situation cannot, or should not, be influenced by the federal government. For example, Brody and Sniderman (1977) argue forcefully that self-interested voting ought to occur only among people who feel that government has clear responsibilities on the issue in question, which turns out to be only a minority of those mentioning a "personal problem" in the 1972 study. In the case of busing, Nixon was clearly perceived as more anti-busing than McGovern (23 and 54 percent, respectively, saw them as supporting it). Relatively few mentioned busing as the single most important national or personal problem. Otherwise the present study offers little empirical evidence available to help us push into these additional possibilities: for example, we do not know to what extent Nixon was generally perceived as capable of influencing busing policy.[12]

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[11] As already indicated, self-interest did have stronger effects on preferences regarding Proposition 13, which involved clear and concrete consequences (Sears, 1978).

Symbolic Politics. In almost all the studies cited above as yielding weak self-interest effects, symbolic attitudes had a vastly greater impact on the formation of policy attitudes (or on voting behavior). As would be expected, racial intolerance carries the most weight on racially related issues; conservatism on racial, crime, and economic issues; and party identification on economic issues. The present study shows this clearly; racial intolerance and political conservatism contribute almost all the variance accounted for in the present analysis, while self-interest contributes almost nothing.[13] However, the role of symbolic attitudes does vary across issues, so a few final comments about "symbolic politics" are in order. The notion of "symbolic politics," as we are using it here, simply means that the ongoing flow of information about public events presents symbols which evoke habitual emotional responses from the citizen. These need not necessarily be isomorphic; symbols similar to those originally associated with the response may, by stimulus generalization, evoke the same response. Hence it is not necessary that today's white adults have been socialized to oppose "busing" as children. The racial imagery surrounding the concept of "busing" is enough to evoke racial attitudes. The clearest cases of "symbolic politics," will be, therefore, those which present the symbols which are most similar to those of original socialization, and which elicit the strongest responses.

It is therefore perhaps not surprising that, in the current line of work, symbolic attitudes have had the strongest effects on explicitly racial, partisan, and economic issues, since racial prejudice and party identification are clearly among the strongest residues of Americans' early political socialization. The weakest effects of symbolic attitudes have occurred with the Vietnam War (Lau, Brown, and Sears, 1978) and the energy crisis (Sears et al., 1978), neither of which seemed to evoke very clear and consensual meaning to Americans. Hence they were not as easily related to symbols which would evoke, on a common, mass basis, the residues of earlier socialization. Such speculations, however, clearly rest upon some unexamined assumptions about how voters process incoming political information.

Busing. "Busing" has become a highly charged symbol of race mixing and of change in the status quo, and so it seems to trigger most opposition from the usual opponents of such social changes, rather than any unique reaction from people who might be personally affected. Similar findings have appeared in other recent studies on the busing issue, whether national or local, as already indicated. Still, it might be thought that the most intense local controversies, such as in Boston, Detroit, or Louisville, might divide whites along self-interest lines. But apparently it does not do so very strongly. In both Louisville and Los Angeles, prejudice and conservatism were by far the strongest predictors of opposition to busing, though in the latter case self-interest did finally have a significant effect (McConahay and Hawley, 1977; Allen and Sears, 1978). So these findings appear to be quite strongly replicable.

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[12] We have subsequently tested several of these hypotheses using the 1976 Center for Political Studies election study, using four policy issues: busing, government-guaranteed employment, national health insurance, and crime policy (Sears, Lau, Tyler, and Allen, 1978). Again, there were minimal effects of self-interest on policy preferences. And contrary to the above reasoning, self-interest did not have stronger effects among the more politically sophisticated, the more selfish, those higher in political efficacy, or those high in perceived government responsiveness. This does not exhaust the possibility that strong self-interest effects may be found under the right circumstances, but it limits the possibilities still further.

[13] It might be noted that the correlations between self-interest and symbolic attitudes are negligible in virtually all these cases. Hence self-interest seems not to influence policy attitudes indirectly by first altering more general predispositions.

The findings leave us with one glaring question. Those opposed to "busing" clearly outnumber the ranks of the racially intolerant and/or the conservatives, by most normal standards. So why is opposition so widespread? It is well known that the amount of racial tolerance varies enormously with the specific area of life in question. There has generally been strong white support for civil rights actions that open up opportunity to blacks (e.g., equal accommodations, voting rights), especially in formalistic, impersonal settings (e.g., formalistic political rights, access to public places, and formal relationships in structured work settings). But tolerance has always been much more limited when intimate personal relationships are involved, such as interracial dating, sex, or marriage (see Triandis and Davis, 1965), or when past ills are to be redressed through preferential treatment (e.g., affirmative action, job quotas, etc.) (Lipset and Schneider, 1978). The latter seem to conflict too much with traditional American values of self-reliance, rewarding "merit," and linking social and economic mobility to hard work. So racial tolerance may perhaps be thought of as resembling a Guttman scale, with many people passing the easy tests, concerning equal opportunity and formal relationships; and many fewer passing the hard tests, concerning preferential treatment in the economic sphere or more intimate relationships.[14]

The symbolism associated with busing may in several respects make it an especially hard test. It of course symbolizes *race mixing*. But beyond that it also symbolizes widespread *forced contact* with blacks, and relatively *intimate contact*, both of which seem troublesome. And it involves the contact of *children* with blacks. Any political issues involving children seem to attract especially protective impulses; e.g., sex crimes, pornography, kidnaping, or political radicalism or sexual deviance among teachers. The mythology surrounding "busing" seems to describe the forced exposure of vulnerable little white children to quite hostile and dangerous black environments. And the way in which "busing" is conventionally presented, in the media or in informal communication, exacerbates the problem. What is conjured up is the picture of a very young innocent wide-eyed (usually blue-eyed as well) child, bravely trudging a block or two to the school in his (more usually her) neighborhood. "Busing" involves placing that innocent child in a far distant blackboard jungle, with switchblade knives, stealing of lunch money, assaults in restrooms, and terror, far from the parent's benevolent protection.

But here it is important to return to our main point, that it is apparently the *symbolism* evoked by the prospect of *any* white children's forced intimate contact with blacks, rather than the *reality* of one's *own* children's contact, that triggers opposition to busing. According to our data, an individual's personal connection with busing seems irrelevant. To be sure, we have no index of concern about those mechanical costs of busing that have nothing to do with race, such as disruption, dollar costs, waste of time, loss of neighborhood solidarity and control over the schools, and so on. But school children are routinely subjected to each of these conditions in situations not involving racial mixing, without anything like the public opposition "busing" has evoked. Long bus rides, consolidated school districts, public schools of mixed social class or of modest quality (at best), peripatetic parents whose children jump from one school or another—all are routine, common experiences in the lives of American school children, without great political outcry. Indeed, one national survey, done in 1972, found the same high level of white opposition to busing to magnet schools ("Would you send your own child to a new and better school in a neighborhood predominantly occupied by residents of another race?") as to busing in general; 75 and 73 percent opposed, respectively (Wall, 1973).

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[14] Additionally, we considered the possibility that the intolerance variable might be multidimensional, rather than unidimensional, as we have been treating it. One strategy was to create an index estimating the second factor found in the principal components analysis described previously. This index, statistically independent of the intolerance scale, measures rejection of black protest versus the other forms of intolerance. This factor did not contribute significantly to predicting the busing item, however, above and beyond the effects of the general intolerance factor. A second approach was to perform a varimax rotation on the entire factored set of racial items. This yielded three clear factors: (1) old-fashioned prejudice (supporting segregated housing, and segregation in general, and believing in racial differences in IQ); (2) opposition to government civil rights action (on public accommodations and fair employment); and (3) opposition to racial protest (feeling that it is moving too fast, is hurting blacks' cause, and is violent). All three related significantly to the busing item, though the protest subscale was weakest. So it does not matter much how the racial items are plugged into the analysis; there appears to be one major factor, generalized racial prejudice, that predicts to anti-busing sentiment.

Moreover, some of these manifest reasons are predicated upon assumptions which are themselves quite doubtful or at least controversial (e.g., the negative impact of school integration upon white children's achievement levels). The average child in any school is rarely victimized by violence;[15] most black children are probably as well behaved in school as most white children are; just as much of the "busing" involves sturdy 17-year-olds as helpless 6-year-olds; more of it usually involves black children than white children; and the intent is to create racially mixed schools rather than to send a lone white innocent into an otherwise all-black ghetto school. And whatever the realities of desegregated schools, it seems clear that most whites are not in a position to have much accurate information about them. Most adult whites have little direct contact with desegregated schools (and indeed, probably very little contact with schools in general). Therefore it seems likely that whites' perceptions of the hazards of "busing" are generally based more on what it symbolizes than on what it actually is.

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[15] This is still true despite the highly publicized recent concern about violence in public schools. Four percent of American teenagers experienced any form of physical violence in school in the year preceding an October, 1977 Gallup survey (Gallup Index, 1977). The 1978 NIE "Safe School Study" found that 1.3 percent of their high school sample reported being physically attacked in a given month. Most of these were minor attacks, though. Only 4 percent of them (or 0.00052 percent of the entire sample) required medical treatment (National Institute of Education, 1978, p. 59). While everyone would agree that in absolute terms this involves too many children, it also means that the jeopardy of any given individual is very low.

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Patrons and Clients, Jobs and Machines: A Case Study of the Uses of Patronage*

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How are patronage rewards allocated within a political machine? This article studies the distribution of 675 CETA Title I jobs within a New Haven machine. Data suggest that the jobs were used as patronage, but that patronage allocations did not follow conventionally assumed patterns of organization maintenance. Ethnic particularism overshadowed, and in fact redefined, considerations of vote-maximization and recruitment of workers. Questionnaire data suggest that those hired were not highly active politically, either before or after hiring, a finding contrary to normal suppositions about patronage recipients. The seemingly anomalous (and perhaps even counterproductive) patronage allocations become understandable, however, viewed in light of some problems and contradictions inherent in patron-client politics. These involve the inflexibility of job-based incentive systems, qualifications on assumptions of reciprocity, and the "aging" of the organization.

The term "political machine" brings to mind a well-ordered, smoothly working patron-client organization. How closely does this image describe actual practice?

This article concerns patron-client politics; its focus is an American urban machine. My primary concern is not the causes of machine politics, nor is it the machine's effects on policy and administration. It is, instead, how rewards and incentives are distributed *within* a patron-client organization. How is patronage used? What principles guide its allocation? What do patterns of reward suggest about the persistence and adaptability of patron-client organizations?

Specifically, I will examine the distribution of 675 patronage jobs within the New Haven, Connecticut Democratic machine in 1974. The jobs were created under the Comprehensive Employment and Training Act (CETA) to provide work experience for the hard-core unemployed. Evidence suggests, though, that they were used as patronage by the party leadership. That machines convert the products of public authority into rewards for the faithful is well known. How these rewards are actually distributed within a machine, however, is much less well known, for as Wilson observes (1961, p. 372, n. 5): "So much secrecy is maintained in city politics that no exact data on patronage may ever be obtained in cities of any size." Much of our knowledge of the uses of patronage is hence highly anecdotal, as, for example, a description of how one ward leader dealt with a handful of jobs, or is derived from a "pure" conception of the machine. On the other hand, Wilson (1961, pp. 372–79) has pointed out potential complications involved in patronage politics. But because data on patronage allocations across whole organizations have been lacking, neither the "pure" models, Wilson's caveats, nor received anecdotes have been put to the test. This article offers such tests, and probes the logic of patron-client politics.

Political Machines and Patronage

A political machine, as found in American cities, is a party organization within which power is highly centralized, and whose members are motivated and rewarded by divisible material incentives rather than by considerations of ideology or long-term goals of public policy. These incentives–money or gifts, jobs, contracts, favors–are used to build an organization which obtains votes (legally or otherwise) in sufficient number to win and maintain control over public authority. Authority in turn is a source of more incentives. This sort of organization has dominated politics at one time or another in many locales, deftly bridging social divisions by seeking money from the affluent and votes from the poor. It has survived–indeed, has appeared to thrive upon–demographic and economic changes which have transformed whole cities. In the face of such trends, machines have been able to "organize, centralize, and maintain in good working condition 'the scattered fragments of power' which are at present dispersed through our political system" (Merton, 1957, p. 72). The use of material rewards has enabled such organizations to do all of this so reliably that they have earned the name "machines."

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Because machines must win elections, and because their internal structures often parallel the city's election districts or neighborhood boundaries, vote production is frequently presumed to be the standard by which machine members are evaluated and rewarded. This supposition is supported by the fact that votes and jobs are both easily counted, meaning that a precinct's job allocation could be made a direct function of the number of votes produced. Ward leaders and precinct captains are often expected, shortly before an election, to estimate how many votes can be got from their bailiwicks. They are then expected to meet those targets. Obviously, precinct captains cannot set targets too high, for votes, not promises, are what count, and they cannot afford to fall short. But neither can they set expectations too low, for exceeding targets by too wide a margin opens them to charges of not knowing what is going on in their precincts, and of leaving results to chance (Rakove, 1975, p. 111). A boss can thus keep track of how many jobs each precinct has, and adjust rewards to performance. Barring severe internal conflict, a "pure" machine could make vote production the sole standard of allocation, and as Wilson notes (1961, p. 373), "Were vote-maximization the single goal, and were the Boss secure in his position, patronage would be allocated to the wards in accord with its marginal product. . . ."

"Pure" machines, however, do not exist. Bosses and followers are human beings, not productive units; they all have class and ethnic attributes, and differ in patience and ambition. All subjectively weigh political work and its rewards against their own expectations, as well as against the machine's standards of merit. Still, one frequently encounters the notion that vote-maximization is at least the *dominant* standard for allocating patronage. (For just two examples, see Kurtzman, 1935, p. 40; and Rakove, 1975, p. 115.) This view holds that the boss simply rewards those who produce votes well, cuts back rewards for those who falter, and replaces those whose performance is altogether inadequate.

Over the long run, however, complications may arise. More jobs may be channeled into marginal precincts than into those returning 90 percent pluralities, in an effort to cultivate new sources of support (Meyerson and Banfield, 1955, pp. 68–69). Politicians have memories; subleaders who helped in key victories years before may still press claims on that basis, especially if the boss came to power at the head of an ethnic faction. Richard Daley took very good care of his "Irish Mafia" (Rakove, 1975, pp. 34–41). Maintaining leadership within the machine, bargaining with other elected officials, and recruiting new workers can all compete (and conflict) with vote maximization as principles of allocation. In the end, the decision as to how to distribute jobs can be quite complicated, as Wolfinger (1973, p. 78) described in the New Haven case:

> The desideratum for all . . . appointments was campaign work for the party, contributions, control of a bloc of votes, or influential friends or relatives. In calculating the relative advantages of different possible appointments, leaders took into consideration such matters as to whom each man owed allegiance, what ethnic group would be "recognized" by his appointment, and whose aspirations could be satisfied or frustrated.

When patronage is allocated across an entire machine, which of these competing principles are most important, and which are peripheral? What are the relative "weights" accorded them? What modifications (if any) of our concepts of patron-client politics are suggested by the answers to these questions? Some preliminary answers are offered in the section to follow.

Patronage Politics in New Haven

The Machine. Richard C. Lee was elected to the first of eight two-year terms as mayor of New Haven in 1953. Lee's win ended the eight-year rule of William Celentano, a Republican whose campaigns had done much to consolidate Italian-American influence in the city (Wolfinger, 1965). The Democratic party had long been Irish-dominated, but faced several long-term problems. Celentano had won heavy Italian support for the Republicans. The city's Irish had long since been eclipsed in terms of sheer numbers by the Italians, and the black community had begun a rapid growth during World War II. In addition, Lee had won on a platform of redeveloping the city's core, an effort which would require many years and the backing of a strong party. The party, then, needed a redevelopment of its own, and a new relationship with the Italian community. For this task Lee and his mentor, John Golden, recruited Arthur T. Barbieri to be Democratic Town Chairman.

Through judicious use of city jobs and the fruits of Lee's $100 million redevelopment program, Barbieri began to build a strong patronage-based Democratic machine. Lee's showings improved in Italian wards, and the party's hold on local patronage grew stronger. By the late 1950s, the organization controlled 800 city jobs (Wolfinger, 1973, pp. 77–78). City patronage was augmented by jobs in redevelopment, the courts, private businesses doing city work, and state patronage, over which the city machine had some influence. Anti-poverty programs in the 1960s created still more jobs.

Poor health forced Golden out of politics in the mid-sixties, and when Lee in 1969 declined to run for a ninth term, Barbieri was alone at center stage. He had spent 15 years building a machine, and was its undisputed boss. Bart Guida, Barbieri's personal candidate, won the mayoralty in 1969 and 1971 over tough primary- and general-election opposition. In 1973 he faced no primary, and only weak Republican opposition, in winning a third term. Barbieri had indeed built a strong machine.

The Data. In the summer of 1974 the city hired 675 young people—most between 18 and 25—for summer work under Title I of CETA. The jobs were funded to provide "the training, education and other services needed to enable individuals to secure and retain employment at their maximum capacity" (Public Law 93–203, Title I, §101, Dec. 28, 1973), but as soon as they were filled, controversy arose. Blacks in particular felt short-changed, contending that few from their communities had been hired despite the aims of the program. Others charged that the Barbieri organization had used the jobs as patronage.

These allegations led the *New Haven Register* to request a list of those hired. After a year's litigation the *Register* obtained the list of names and addresses by federal court order, and published it (26 June 1975, p. 20), together with a city map on which dots represented the home addresses of persons hired. Jobs were indeed thickly clustered in the heavily Italian East Side wards which were the core of the machine. The rest of the city had many fewer jobs, and poor and black districts got but a handful. Most observers agreed that the jobs had been used as patronage. This job distribution can tell us how one machine put one kind of patronage to use. It is hardly a perfect measure of all patronage allocations in the New Haven machine, let alone necessarily representative of allocations in other machines. But the CETA jobs do represent a citywide patronage distribution; since patronage data are so scarce, an analysis of it is worthwhile.

To study this distribution, I scored each of the city's 30 wards (as of 1974) on the number of jobholders living within its boundaries.[1] Since the wards were not subdivided into precincts, they and their Democratic organizations were the building blocks of the citywide machine. I also assembled returns for all mayoral elections, general and primary, from 1969 through 1975, and a student under my instruction mapped census-tract social characteristics (percentages of the population black, of Italian stock, and of Russian/East European stock; median family income, percentage of families under the "poverty line"; and median years of education) into the wards on an area-weight basis. While this last process has its shortcomings, assuming as it does that social characteristics hold uniformly throughout tracts, it still yielded sound ward-level data.

I gathered individual-level data on the jobholders by means of a mail survey. In late 1977, a systematic sample of 135 jobholders, derived by selecting every fifth name on the list of those hired, received a one-page questionnaire requesting information on ethnic background, partisan loyalties, political interest, and political participation before and after hiring. I received some 33 responses; while this is a decidedly modest rate of return (24.4 percent of the sample, and about 5 percent of all persons hired), the data allow tentative observations on the political involvement of those chosen to receive jobs.

For most of what follows, the units of analysis are the ward organizations. I will regard election returns, for example, as indicators of the performance of ward organizations in their task of mobilizing and controlling the vote. In comparing vote totals to job allocations, then, I presume that jobs are being used not to entice voters directly, but rather to reward or penalize ward organizations. This presumption minimizes problems of ecological inference, ties the strategy of analysis into the machine's patron-client structure, and reflects the way in which machines mobilize and channel mass participation.

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[1] The computations reported in sections to follow all employed the number of jobs in the wards as the dependent variable. I also prepared similar computations using variations on this dependent variable, such as a ward's percentage of the total job allocation, number of jobs divided by number of registered Democrats, and jobs divided by number of registered voters. These replications produced results which differed only in detail from those presented in the text, and presented essentially the same substantive findings. For simplicity and ease of interpretation, then, I retained simple numbers of jobs as the dependent variable.

Hypotheses. Three general hypotheses, and a number of more specific ideas, can be advanced to explain this patronage distribution. First (a "null hypothesis") is that the jobs were *not* used as patronage, but were instead distributed according to the intent of Title I. Second, the jobs might have been used as patronage, primarily for organization maintenance. Two possible types of maintenance—vote maximization and recruitment of workers—will be examined. Vote maximization could mean (a) the "ideal" (equal-marginal-product) distribution; (b) distribution based simply on past vote production; or (c) efforts to develop new sources of support. The third major hypothesis involves ethnic particularism: since Barbieri, an Italian-American, built his political base in the Italian wards, perhaps he used the jobs to maintain that ethnic support. These principles of allocation are less discrete in practice than in theory: rewarding active Democrats, for example, would entail rewarding many Italians. Still, the data allow us to test hypotheses against the actual job allocation.

Hypothesis 1:
Original Intent of Title I

Were the CETA jobs actually used to fight hard-core unemployment? The data suggest they were not. The simple correlation at the ward level between number of jobs and median family income was +.20 (p = .143); between jobs and percentage of families below the 1969 "poverty level," −.41 (p = .013); and between jobs and percentage black, −.50 (p = .002). These results, of course, are based on characteristics of areas and not of people, but if jobs had been channeled into high-unemployment districts we would not have obtained these correlations. The list of persons hired gave no information on race, but only two of the questionnaire respondents (6.1 percent) were black. Further, the ward with the highest number of jobs (72) had the third-lowest percentage of "poverty families," and third-highest median family income, in the city. The three wards with highest poverty concentrations, by contrast, received only 12, 5 and 4 jobs each. It is conceivable that jobs went to poor individuals living in affluent areas, but the overall distribution suggests that the goals of Title I did not guide job allocation.

Hypothesis 2:
Organization Maintenance

Vote Maximization. The "ideal" vote-maximizing distribution is that which produces an equal marginal vote payoff wherever a job is allocated. Even if Solomon had his own political machine, however, he would find this distribution elusive, for the intangibles of the electoral process are many. Thus it is not surprising that the New Haven job distribution was far from ideal. In the first mayoral election after the job allocation (the 1975 primary), the 30 ward organizations produced an average of 36.4 Guida votes per job. But the standard deviation around that mean was 46.9; the wards ranged from 7 votes per job (three wards) to highs of 185 and 214.

The 1975 primary was hotly contested, with some ward organizations supporting Guida's (ultimately successful) challenger. So I made similar calculations for the 1973 general election, the last before the job distribution and one in which the machine was united behind Guida. For that race, the 30-ward average was 101.1 votes per job. But the standard deviation was 137.4, with three wards producing as few as 15 votes per job, seven over 100, and two as many as 509 and 636. These last figures, of course, reflect low patronage allocations, not high productivity per se. But such differentials suggest not only that the ideal allocation was not achieved, but also that it was never intended. Similar disparities were found in all mayoral elections between 1969 and 1975.

Past performance would be a simpler way to make vote production the standard for patronage allocation, and here the evidence is more positive. Simple correlations between the Guida vote totals and the 1974 job distribution appear in Table 1.

The job distribution partially corresponded to patterns of past support. This is not surprising, but further examination suggests that it is not the whole story. Two of Guida's challengers over this period—DiRienzo, a minor-party candidate in the 1969 general election, and Esposito, the Republican nominee in 1973—had *higher* correlations between their support and the job distribution than did Guida in the same election (DiRienzo, 1969, +.71, p = .001; Esposito, 1973, +.64, p = .001). Another challenger in the 1969 general election had a correlation which fell just short of Guida's. Guida's *totals* were higher, even when correlations were lower, and I am not suggesting that jobs were channeled to opponents. Still, it is noteworthy that in 1973, with Guida running at the head of a united machine, the opponent's vote was more strongly correlated with patronage distributions. And if in 1973 Guida won support from non-machine Democrats who could not bring themselves to vote Republican—thus watering down his correlation—why then in 1969, against stiff opposition in both rounds, was a similar result obtained?

Let us look more closely at patterns of machine support. In the 1973 general election, Guida ran well in many parts of the city. Two of his most important sources of support were the "Italian wards" on the east side, and heavily black wards on the west. Comparison of support and patronage between these areas is revealing. Consider six west side wards which collectively ran well ahead of the rest of the city in Guida support (72.9 versus 60.6 percent city-wide). Three of these wards had black majorities, and two of the other three were over 30 percent black. As a group, they gave Guida 16.0 percent of his total vote, but received only 7.1 percent (48) of the jobs. Had jobs been distributed solely on the basis of Guida support, these wards would have received 60 more jobs. By contrast, nine east side wards, with the city's heaviest concentrations of Italian-Americans,[2] actually ran *behind* the citywide Guida percentage, 56.2 to 60.6 percent. While 35.2 percent of the Guida vote came from these wards, they received 53.9 percent (364) of the jobs. Had jobs been distributed according to Guida votes only, these wards would have got 126 fewer jobs.

Thus the machine's "core wards" were actually over-rewarded for their support, while others were under-rewarded. This pattern was found, in varying degrees, for these groups of wards in all elections during the Guida years. That job allocations only imperfectly reflected past support was also shown by a stepwise regression of the job distribution on all five previous Guida campaigns. This produced a corrected R-square of .66 (p = .01); but because election results were strongly collinear, nearly all of this R-square was accounted for by the first election to enter, the 1969 primary. The last three to enter actually had negative Betas. So past performance, while significant, does not fully explain the distribution of jobs.

Perhaps jobs were used to cultivate new areas of support. To test this notion, I correlated the job distribution with a "new votes" measure derived by simply subtracting Guida's 1971 primary totals from his 1975 primary results. A strong positive correlation would have been consistent with a "new support" hypothesis, but in fact the correlation was −.35 (p = .029). If anything, the best-rewarded ward organizations fell down on the job.[3] It is also

Table 1. Simple Correlations between Guida Vote Totals and 1974 Job Distribution

Guida Vote in:	r	p
1969 Primary	+.76	.001
1969 General	+.52	.002
1971 Primary	+.72	.001
1971 General	+.71	.001
1973 General	+.52	.002

Source: Author's count of CETA jobs by wards, based on list of names and addresses of persons hired, *New Haven Register*, 26 June 1975, p. 20, and on official election returns, Office of the Registrar of Voters, City of New Haven.

Note: Probabilities shown are results of one-tailed T-tests.

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[2] Census data from 1970 suggested that first- and second-generation Italian-American percentages ranged from 18 to 32 percent in these wards; data on third- and later-generation Italians, were they available, would add significantly to these percentages. These wards had mostly Italian-American ward leadership.

[3] Another test of this hypothesis was made through a multiple regression technique designed to estimate the amount of variance in the 1975 Guida vote "explained" by the job allocation *independently* of the 1969 and 1971 patterns of support. Using the 1975 primary vote for Guida as the dependent variable, Guida's 1969 and 1971 primary totals, plus the job distribution, produced an R-square of .77 (p = .001):

	Beta
Guida 1969 primary	.18
Guida 1971 primary	.61
Job distribution	.14

Sorting out the independent contribution of the jobs variable to this R-square is difficult, since it is partly collinear with the other two independent variables. Therefore, I prepared two more equations; in the first, the two previous elections were entered first as a group, followed by jobs, while in the second the order was reversed. The first equation arbitrarily assigns the collinear portion of the R-square to the two "control" elections, while the second assigns it to the job distribution. Observation of R-square increments thus provides estimates of the upper and lower limits of the contribution to R-square by the two sets of independent variables:

	Minimum	Maximum
Two "control" elections	.268	.763
Job distribution	.008	.503

These estimates suggest, but of course do not prove, that the job distribution made a relatively small worth noting that the 1975 primary had the lowest correlation with the job distribution of any of Guida's primary showings.

The "new support" hypothesis can be refined: the most likely new constituency to recruit would have been blacks. Ordinarily this would not have been the case, but in the 1969 and 1971 Democratic primaries Henry Parker had mounted the city's first serious black mayoral campaigns. Although he lost to Guida in both races, Parker ran very well in black areas. The demonstrated ability of a black candidate to mobilize the growing black vote posed a threat to the Barbieri machine. In 1973, however, Parker publicly supported Guida, a move which directs our attention to the 1974 job allocations. Were jobs used to mend fences with blacks?

The data are clear: the jobs were not so used. I do not know the number of blacks hired, but we have already seen that black areas were consistently under-rewarded for their support. Data on ward social characteristics reflect a similar pattern. Simple correlations have shown that black and poor areas received few jobs. Stepwise regression of jobs on the wards' percentage black, median family income, percentage of poverty families and median years of education showed that percentage black entered first, with a negative Beta ($-.50, p = .01$). With this, median years of education showed a negative partial correlation larger than its original simple correlation (simple $r = -.32$, $p = .04$; partial $r = -.46$, $p = .01$), raising the interesting possibility that the job allocation conformed slightly more closely to Title I goals after percentage black is controlled. We cannot exclude the possibility that jobs went to blacks living in mostly white areas, but if jobs had been channeled to the ward organizations best able to mobilize the black vote, we would not have obtained these aggregate results.

Recruiting New Workers. That patronage recipients often become precinct workers is a key supposition in most accounts of patronage politics. I gathered questionnaire data to test this assumption. While the small sample does not provide a complete picture of job recipients' participation in politics, it seems unlikely that receiving a job produced marked increases in activity. Results of two questions, asking respondents to describe their political activities before and after being hired, are shown in Table 2.

There was no massive increase in political involvement by the respondents. Levels of participation, in fact, are unimpressive both before and after hiring. The data become an index of participation ranging from 0 to 12 if

Table 2. Political Involvement of Respondents before and after Hiring

Q: "*Before you were hired by CETA*, how often did you take part in the following political activities?"

	Never	Seldom	Often	Almost Every Election
Voting	81.8%	6.1%	3.0%	9.1%
Wearing a button, displaying a sign	54.5	27.3	15.1	3.0
Doorbell-ringing, poll-watching, etc.	63.6	27.3	6.1	3.0
Donating money to candidates	90.9	6.1	3.0	–

Q: "*After you were hired by CETA*, how often did you take part in the following political activities?"

	Never	Seldom	Often	Almost Every Election
Voting	57.6%	9.1%	6.1%	27.3%
Wearing a button, displaying a sign	57.6	24.2	12.1	6.1
Doorbell-ringing, poll-watching, etc.	54.5	30.3	9.1	6.1
Donating money to candidates	84.8	12.1	3.0	–

Source: Author's mail survey of persons hired under CETA Title I, City of New Haven, 1974. Names and addresses of persons hired were published by the *New Haven Register*, 26 June 1975, p. 20. Survey was mailed to a systematic 20 percent sample of the 675 hirees (every fifth name on list, N = 135); percentages above based on return N of 33 (24.4 percent of sample).

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contribution to the 1975 vote. Taken together with the negative correlation reported in the text, these results cast doubt on the hypothesis that the job distribution won significant new support for the machine.

we score the types of activity from 0 to 3 in order of increasing frequency and sum the results. Measured thus, the sample's average participation score was 1.61 before hiring, and 2.54 after (one-tailed $p = .012$). Despite the statistical significance of the increase, as a practical matter these respondents did not become an army of willing political workers: on this scale, a group which voted every election but did nothing else would have a mean of 3.00. Indeed, 71.0 percent of the increase in participation (measured on the 12-point scale) came in the form of more frequent voting. Only a small increase was reported in canvassing and poll-watching, the mainstays of machine campaigning.

Thus, while a few individuals may have increased their political activity after hiring, as a group the respondents do not conform to the traditional image of patronage recipients. Moreover, I question just how much of the reported increase is the result of having received a job. Maturation of respondents is a factor here, since job recipients were young people. We would expect *some* increase in participation, over time, in *any* sample of this age group. These data probably overestimate to some degree the effects of patronage on participation.

Hypothesis 3: Ethnic Particularism

Ethnicity is important in New Haven politics (Dahl, 1961; Wolfinger, 1965, 1973; Johnston, 1977). Barbieri's success as party chairman rested heavily on his ability to bring Italian-Americans back into the Democratic fold. Perhaps an Italian-led machine simply distributed jobs to Italians, and perhaps this principle of allocation was significant independent of considerations of direct organizational maintenance. Let us examine these possibilities.

Italian-Americans were well rewarded in the job allocation. Of the 675 persons hired, 386 (57.2 percent) had Italian surnames; New Haven is about 35-40 percent Italian. There were, by comparison, 41 Irish (6.1 percent) and 50 Russian and East European surnames on the list (7.4 percent). Surnames are not infallible indicators of a person's background or self-perceptions, but the preponderance of Italian surnames is nonetheless striking. Similar evidence comes from stepwise regression of the job distribution on ward social characteristics. Table 3 shows the equation after step three.

Subsequent steps picked up three additional social characteristics, but none entered with a significant *F*, and none altered the importance or interrelationships of the first three independent variables. Percentage Italian is clearly the social characteristic most closely related to the job distribution. These are data on areas, not people, but given the preponderance of Italian surnames on the list of persons hired, it is not unreasonable to conclude that an Italian-led machine channeled this sort of patronage primarily to Italian-Americans.

But did ethnic particularism outweigh considerations of organization maintenance? If Italian-American job recipients reported higher levels of participation before hiring, or larger post-hiring increases, than the rest of the group, then perhaps they won their jobs through their political efforts rather than by ethnicity alone. Or, ethnicity and political effort may have at least been parallel principles of allocation. Table 4 compares respondents who labeled themselves as Italian-American and as supporting Guida in the 1975 primary—the machine's "core constituency"—to other respondents:

Evidence that ethnic particularism really was produced by greater Italian-American participa-

Table 3. Results of Stepwise Regression of Job Distribution on Ward Social Characteristics (after step three)

Dependent Variable: Jobs Independent Variables:	Beta	*F*	*p*
Percent Italian-American	+1.03	73.97	.001
Median Family Income	+ .33	9.12	.01
Percent Black	+ .28	4.21	.02

Adjusted $R^2 = .78$
$F = 36.11, p = .01$

Source: Jobs variable derived from author's count of CETA Title I jobs by wards, based on list of names and addresses of persons hired, *New Haven Register,* 26 June 1975, p. 20; ward social characteristics derived from census-tract data, 1970 census, mapped into wards on a block-by-block area-weight basis.

tion is only very weak here. The "core constituency's" means and increase are relatively large, but represent very low absolute levels of involvement. These are hardly the levels of involvement one expects from true machine stalwarts. Ethnic particularism seems significant independent of actual political involvement.

Were "Italian wards" so well rewarded simply because of vote productivity? I have already noted that Italian ward organizations were over-rewarded by comparison to actual levels of support. Correlation and multiple-regression analysis also suggest that ethnic particularism was not merely an artifact of patterns of vote production. Guida's totals in all of his campaigns were positively correlated with the job distribution (Table 1), but no correlation was as high as that between jobs and percentage Italian in the wards ($r = +.86$, $p = .001$). In a stepwise regression of jobs on six ward social characteristics and all of Guida's totals, the results after three steps were identical to those in Table 3. Only on the fourth and fifth steps did any Guida vote totals enter; these (from the 1971 primary and 1973 general election) were statistically insignificant. In the end, several kinds of stepwise regression and partial-correlation analysis failed to displace percentage Italian in favor of any Guida vote totals.

Ethnic Particularism: Redefining Productivity

The 1975 elections showed that vote production, as a principle of job allocation, carried most weight when viewed within the context of ethnic particularism. In that year Guida lost a bitterly contested primary to Frank Logue, a white reform Democrat. This race split the party along Italian/non-Italian lines in much of the city. In the general election, Logue won a narrow victory over Republican John Esposito, at the time head of the local Italian-American Civil Rights League. Barbieri assumed a public stance of neutrality after the primary, but in fact actively aided the Republican. Barbieri's efforts helped Esposito run the strongest GOP mayoral race in years, gaining him much support in the "Italian wards." With these events in mind, consider the correlations in Table 5.

The 1975 results present the unusual picture of Democratic patronage being largely unrelated to Democratic voting, while being strongly correlated with patterns of Republican strength. Barbieri and his strongest ward organizations produced this result. In Barbieri's home ward alone, Esposito nearly doubled his 1973 vote, and his percentage rose from 44.8 to 72.6 percent. Esposito's vote, and his 1973–75 "swing," are more closely correlated with the job distribution than any other measure of vote production. Barbieri's contribution to this "swing" is underscored by the fact that while Esposito's 1973 totals correlated +.87 and +.82 with the 1969 and 1971 showings of another Italian Republican, the 1975 correlations dropped to +.59 and +.51, respectively. Esposito's "swing" correlated only +.05 with the 1969 GOP totals, and –.04 with 1971. Esposito's vote not only increased, but also changed markedly in its distribution, thanks to Barbieri.

Stepwise regression highlights the relative importance of ethnic particularism. I regressed jobs on a long list of independent variables, including ward social characteristics, all Guida vote totals, the 1975 Logue-Esposito results, and the 1973–75 vote "swings" for Esposito and Guida. Results of the first four steps of the equation are reported in Table 6.

Here for the first time a measure of vote production—the 1975 Esposito vote—takes precedence over percentage Italian. Not all of Esposito's votes were produced by the Barbieri organization, but as we shall presently see, the

Table 4. Pre- and Post-Hiring Participation Scores, "Core" and "Non-Core" Constituencies

	Mean Participation Score *before* Hiring		Mean Participation Score *after* Hiring
"Core constituency" (12) (Italian *and* pro-Guida)	2.08	← $p = .044$ →	3.58
	↑ $p = .135$ ↓		↑ $p = .044$ ↓
"Non-core" (21)	1.33	← $p = .079$ →	1.95

Source: Participation scores based on author's mail survey of persons hired under CETA Title I, City of New Haven, 1974. Names and addresses of persons hired were published by the *New Haven Register*, 26 June 1975, p. 20. Survey was mailed to a systematic 20 percent sample of the 675 hirees (every fifth name on list, N = 135); scores above based on return of N of 33 (24.4 percent of sample).

Note: Probabilities shown are results of one-tailed T-tests.

machine had its links with the primarily Italian GOP leadership. The weight of the percentage Italian variable, of the 1975 Esposito vote viewed in the context of the events of that year, and the negative Beta of the only Guida campaign to enter the equation, combine to suggest that ethnic particularism was the single most important principle of patronage allocation.[4]

Table 5. Correlations between Job Distribution and 1975 Mayoral Results

Jobs and:	r	p
Guida, 1975 primary	.71	.001
Logue, 1975 primary	–.16	.198
Logue (D), 1975 general	–.20	.139
Esposito (R), 1975 general	.85	.001
Esposito "swing," 1973–75	.83	.001
Esposito "swing" as percent of registered Democrats	.83	.001
Percent Italian-American	.86	.001

Source: Jobs variable is from author's count of CETA Title I jobs by wards, based on list of names and addresses of persons hired, *New Haven Register*, 26 June 1975, p. 20; election results derived from official election returns, Office of the Registrar of Voters, City of New Haven.

Note: Probabilities shown are one-tailed.

Vote production on behalf of Italian-American candidates endorsed by an Italian "boss," then, was a more critical standard of productivity than votes produced for Democrats per se. Vote maximization works best as an hypothesis, in this case, *when defined and analyzed in the context of ethnic particularism.* I do not claim that Barbieri in 1974 was able to predict events 15 months hence and award jobs accordingly. Rather, the ability of a ward organization to swing Democratic votes to an Italian Republican depended upon a complex set of sentiments and linkages at all levels—at the top of the machine, in the ward organizations, and among voters in the "Italian wards"—which can best be termed ethnic particularism. That this is a part of machine politics is hardly a novel finding; the Irish and others have followed this principle as well (Cornwell, 1960, 1969; Holden, 1973, Ch. 6). But that ethnic particularism is strong enough to outweigh and even redefine other principles of allocation seems remarkable indeed. More will be made of this in a concluding section.

The importance of ethnic particularism in Barbieri's organization is borne out by a brief look at other types of patronage. It may be most accurate, in fact, to regard the organization as an Italian machine: between 1953 and 1971, the number of non-elected full-time city employees with Italian surnames increased from 99 to 214 (Lopatin, 1975, p. 43). The Democratic party was the prime component of this machine, holding a five-to-one edge in voter registration; since Connecticut law provided for closed primaries, the party label was a valuable asset over which Barbieri had much control. But an "Italian machine" could easily cross party lines; indeed, it had to do just that, for there were still many Italian Republicans. And if GOP opposition could be reduced by interparty initiatives, Barbieri's own position would be all the stronger. One local newspaper account (Houlding, 1976) had it that Barbieri built a "double machine," through which he "could provide patronage plums to a few key Republican leaders to reduce the intensity and effectiveness of their campaign opposition."

In 1965, the report noted, Barbieri played a role in the nomination of an inexperienced Republican mayoral candidate. In 1967, he was "friendly" with another GOP nominee, and ordered his lieutenants to cut Mayor Lee from the ticket in a few areas, splitting instead for the Republican. During the Guida years, Barbieri was on good terms with the Republican chairman, himself of Italian ancestry. When Guida's administration came under fire over the management of a Model Cities program, Bar-

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[4] I put this assertion to an additional test by selecting the "best" independent variable for each of the three main hypotheses and running a series of equations to estimate maximum and minimum contributions to R-square for each. The method was similar to that described in footnote 3, and yielded the following estimates:

Hypothesis*	Variable	Min.	Max.	Mean
(1)	Percent Italian	.177	.738	.446
(2)	Median years education	.009	.100	.042
(3)	Guida 1969 primary	.015	.573	.282

*Hypotheses: (1) Ethnic particularism
(2) CETA purposes
(3) Vote production

The figures in the final column (the mean contribution across the six equations which were used to enter the variables in all possible orders) should be viewed with caution, since they represent assumptions about the apportionment of multicollinearity no less arbitrary than those underlying the minimum and maximum figures. Still, the data do suggest again the relatively great importance of ethnic particularism as a basis for job allocation.

bieri reportedly helped swing a $30,000 Model Cities auditing contract for the firm employing the Republican chairman. He also backed a Republican attorney as part-time Model Cities counsel. The GOP platform in the next election, written by the attorney and the party chairman, made no mention of Model Cities (Houlding, 1976).

Double-machine politics and ethnic particularism are hardly unique to New Haven. But when Barbieri in 1975 saw fit to support an Italian Republican, he could do so knowing that the patronage he had channeled to Italian-Americans in the GOP would make his move easier and more effective. The 1975 election thus bears witness to an ethnic particularism which was many years in the making, and which was a fundamental orientation of this machine. These findings have implications for our understanding of patron-client politics in many settings, and for wider issues of urban politics. These are the subject of the concluding section.

Conclusions

Have we studied an example of the use of patronage, or of its *misuse*? Barbieri's ethnic particularism antagonized many non-Italians, perhaps needlessly, and may well have contributed to the defeats of 1975 and to Barbieri's loss of the party chairmanship a few months later. Perhaps we are searching for "principles of allocation" in vain. Perhaps this is a case in which—with apologies to G. W. Plunkitt—a boss simply "seen his opportunity and blew it." No one who watched Barbieri in action, though, could doubt his political skills. Instead, the CETA patronage case suggests a need for some revisions of our picture of machines. "Inexplicable" results may actually follow from three unrecognized problems involved in patron-client politics.

1. Patronage jobs, as divisible material incentives, are rather awkward and inflexible tools for maintaining an organization. They are much more useful in starting an organization than in adapting it to changing circumstances.

If I wished to build a strong, adaptive machine, I would want to use divisible material incentives numerous and valuable enough to motivate my supporters, but sufficiently finely

Table 6. Results of First Four Steps of Regression Analysis

Dependent Variable: Jobs Independent Variables:	Beta	F	p
Step one:			
Percent Italian-American	.86	78.95	.001
	(R^2 = .74; Adj. R^2 = .73; p = .001)		
Step two:			
Percent Italian-American	.50	12.13	.01
Esposito 1975G	.45	9.65	.01
	(R^2 = .81; Adj. R^2 = .79; p = .01)		
Step three:			
Percent Italian-American	.51	14.56	.01
Esposito 1975G	.61	15.67	.01
Percent black	.25	4.74	.01
	(R^2 = .84; Adj. R^2 = .82; p = .01)		
Step four:			
Percent Italian-American	.40	6.66	.01
Esposito 1975G	.87	13.82	.01
Percent black	.24	4.41	.01
Guida 1973G	−.21	2.05	.08
	(R^2 = .85; Adj. R^2 = .83; p = .01)		

Source: Jobs variable is from author's count of CETA Title I jobs by wards, based on list of names and addresses of persons hired, *New Haven Register*, 26 June 1975, p. 20; election results derived from official election returns, Office of the Registrar of Voters, City of New Haven; ward social characteristics were derived from census-tract data, 1970 census, mapped into wards on a block-by-block area-weight basis.

Note: No variable entering after step four produced a statistically significant F, and none altered the interrelationships of the four independent variables appearing above.

divided to allow precise adjustment or reallocation of rewards. We often assume that the boss' patronage resources meet both standards. In practice, however, these goals are contradictory.

I might, for example, try to build a machine using small peppermints as incentives. Peppermints are numerous and renewable; they are also so finely divided that I could easily adjust allocations in proportion to performance, or in reaction to events. Productive precinct captains would get more peppermints, while slackers would suffer a cutback reflecting my displeasure, but could still receive enough to prevent serious resentments. The problem here is obvious, though: however precise and adjustable peppermint patronage might be, few people would do the hard work of a precinct captain in exchange for candy.

Realizing this problem, I could go to the other extreme and reward my backers with Cadillacs. Most people would ring many doorbells in pursuit of such a reward. Over time, though, I would find it hard to adjust rewards to performance. If some workers' productivity began to slip, I could take away their cars or pass them by if I should hand out a second round of Cadillacs. But the former step, legality aside, is a rather severe penalty to exact merely for a moderate decline in performance; in a close election, in fact, I might not do it for fear of antagonizing such persons and their followers. The latter tactic would exhaust my resources very quickly. A machine based on Cadillacs, then, would be an inflexible one.

The point is that jobs leave much to be desired as organization-maintaining incentives. They are probably more like Cadillacs than peppermints, but even if one can motivate people by handing out jobs—an assumption which the questionnaire data suggest needs reexamination—once a job has been awarded, the boss' options for making adjustments in rewards are limited. A jobholder who pleases the boss can be given a raise or a better job, but such increments may be limited, across an entire machine, by the size of the public budget or some irreducible level of services which must be provided. In dealing with slackers, options are even fewer; ultimately the choice may be between the relatively extreme step of taking away a job, or deciding to tolerate suboptimal levels of performance. This is not to say that bosses do not take jobs away, for they can and do; rather, it is to say that sizeable gaps between patterns of reward and performance should not be surprising. The larger the individual reward, the greater its value, but the "stickier" it becomes in change and transition. As a result, jobs are probably much better suited to establishing a machine than to adapting and maintaining it. (Of course, jobs are not the only rewards at a boss' disposal. But for the core of the organization, jobs are the mainstay of the patronage system.)

A related problem with jobs is their scarcity. No boss ever has enough to go around; not only are the sources limited, and often encumbered by civil-service regulations, but if jobs are indeed hard to reallocate, scarcity may be more of a continuing problem for the leadership than an inducement to sustained effort for the rank-and-file.

2. Networks of obligation and reward will be much more complex in practice than in theory, and will be based on many standards of exchange.

A straight reward-performance tradeoff would suggest that simple reciprocity is the machine's unifying principle. But machines are too large and complex for this; after all, a perfectly reciprocal machine could hardly have a boss. What are the implications of dropping the assumption of reciprocity?

Bosses remain bosses by maintaining an imbalance of obligations in their favor. Simply put, most of a boss' followers feel they owe the boss something, or at least that they are obliged to support the boss actively if they are to win future benefits. Clearly this is not a simple reciprocal arrangement. But bosses depart from reciprocity in other ways. They can monopolize information on patronage, so that subordinates do not have any access to any "central account" of performance and rewards. It is arguable that the New Haven machine leadership fought the release of CETA data not for fear of public reaction—the results in fact surprised very few—but rather because factions and ward organizations would be able to compare their rewards with those of others. The absence of such a mutually acknowledged "central account" enables a boss to set unilaterally, and to vary from faction to faction, the "exchange rates" of performance and reward. Notions of reciprocity may enter into these "rates," but can vary among subgroups; perhaps it is useful to think of *multiple* reciprocities. Arbitrary and multiple exchange rates give the boss a great deal of discretion in the short run, but—as we shall presently see—can make for organizational inflexibility over the long run.

With simple reciprocity disposed of, many other principles of allocation begin to figure into patronage calculations, and many kinds of claims other than productivity are made upon the boss. Sub-leaders might have clout in their own right, or might be expected to carry extra-large burdens at election time; in either case they might be over-rewarded. Certain ethnic communities, or areas showing signs of independence, could also require special treatment. If the inflexibility of jobs as incentives makes a direct performance-patronage exchange unlikely, then recognizing that simple reciprocity need not prevail opens the door for many other considerations to become routine bases for patronage allocation.

3. Patron-client organizations tend to age, just as people do.

The "pure machine" is a timeless creation. But if job-based patronage adapts only awkwardly and multiple reciprocities inject considerations other than productivity into patronage allocations, then it follows that machines can "age" (McKitrick, 1961, p. 456). This can happen in at least two interrelated ways: participants themselves may age, or the machine's leadership, factional structure and patterns of reward may fail to adapt to new sociopolitical trends. Margolis, Weinberg and Ranck (1977) have described the consequences of individual aging in the Pittsburgh Democratic machine; the latter type of aging is best exemplified by the Chicago Irish hegemony. Aging can further reduce the importance of vote-maximization, and emphasize ethnic and other claims in patronage allocations, for several reasons. First, the machine is a continuing entity; it is not constructed de novo for each election or intraparty fight. Thus when bosses ask what their followers have done for them lately, machine workers may well cite their deeds over a number of years. Second, a machine's leadership often comes to power through some decisive victory. Key backers from those early struggles can become a "core faction" in a machine, and can make strong claims on rewards long after the fact.

Claims based on past efforts would carry little weight in a strictly reciprocal organization. Ironically, though, the same lack of standard "exchange rates" which gives the boss maneuvering room in the short run can be an encumbrance in the long run. While in the short run no subordinate can authoritatively claim to have earned a reward, over the long run the boss may be unable to declare old accounts "closed" without a fight, unless the claimant leaves the organization or drops the claim. Hence, old debts may be paid, repaid, and repaid again, long past the time when considerations of organization maintenance have changed. The more important the faction, the fewer may be the boss' choices in such matters; old "core factions" may thus be routinely over-rewarded, while newer subgroups go under-rewarded. This state of affairs may indeed suit the boss just fine; machine elites frequently resist all but the smallest political changes. But even if it does not, it can be most difficult to adapt patronage to new needs. Factional fights often grow out of just this sort of "recognition" problem.

The seemingly puzzling New Haven patronage patterns, then, may grow directly out of, and illuminate, some of the inherent limitations of patron-client politics. The "lumpiness" of jobs as a patronage resource prevents direct patronage-performance tradeoffs; "multiple reciprocities" allow the boss flexibility of allocation in the short run; but with the aging of the machine, ethnic and other non-performance considerations can move to the fore, and the boss' choices may become fewer and fewer.

General Implications

Patronage politics has not withered away. If one jobs program in one middle-sized city generates 675 jobs for a local party, the patronage sources can hardly be said to have dried up. Indeed, the New Haven case highlights the importance of intergovernmental patronage sources. There are fewer classic centralized machines in American cities than once was the case, but patronage politics as a process is still important in dozens of cities and many states (Wolfinger, 1972). Patron-client organizations in other nations, including rapidly urbanizing third-world regions, have recently come under increased study (Sacks, 1976; Scott, 1972).

But patron-client politics is not the smoothly functioning, almost self-starting process we often assume it is. Shefter (1976) reminds us that machines did not automatically spring out of a sociopolitical context, but were instead put together by real people making real choices. The same is true of maintaining a machine, and this analysis, like Wilson's (1961), suggests that patronage allocation can be far from simple. Principles of allocation conflict, adjustment of patronage is not always easy, the value of performance in terms of reward is always open to dispute, and future needs become obscured by old debts. Complications, it seems, are the rule rather than the exception.

These complications raise questions about other more general issues. Sometimes we are told that machines at least "make cities work," and that their return would thus be a welcome development. It is indeed true that "reformed" governments have at times been worse than the ills they were supposed to have cured (Lowi, 1968; Lineberry and Fowler, 1968). But if machines are as slow to adapt as we have suggested, and if problems involved in their operation range from complex to insoluble, then is such a blanket prescription warranted? If Chicago's government really "works" better than others (a proposition I have yet to see demonstrated), does this tell us anything basic about patron-client politics? Or is it more likely a result of one boss' particular skills, or indeed of the limited duties of that local government? A patron-client organization may be an effective way to win and hold power, but the way that power is used depends upon the full range of factors shaping urban politics. This issue is critical in rapidly urbanizing nations. How well will patron-client organizations govern their cities? The answer to this question will outline the urban future of much of humanity.

We are also told that machines hastened the upward mobility of poorer classes, and dampened hostilities among ethnic groups, by treating all as potential blocs of support. Specifically, the absence of classic machines has been proposed as one cause of ghetto rebellions and the continuing problems of urban minorities (National Advisory Commission on Civil Disorders, 1968, p. 287; Scoble, 1969, pp. 649–51). The importance of ethnic particularism in the New Haven case, however, suggests that if a machine is to benefit blacks, for example, it had better be a black-led machine. Machines may have facilitated upward mobility for individuals and small factions, but never for entire ethnic "out-groups." Indeed, they never could have, for to do so would have upset the machine's delicate balance between the mass support of the poor and the financial support of business "clients." Machines dispensed favors as needed to satisfy the sorts of internal needs discussed in this article but never indulged in social services for their own sake.

Also interesting is the imperfect correspondence between rewards and support for the organization. Whether for lack of data or other reasons, we often assume that patronage produces support in a simple, more or less direct manner. Yet our data, like Sorauf's (1956) study of patronage workers on Pennsylvania highway crews, suggest that patronage does not necessarily produce high levels of active support. Just how representative these findings may be, and why this key supposition of the machine literature may not hold true, are interesting questions for further work. And how to account for the support of those who get little? Political culture and perceptions of actual and appropriate rewards—as well as more immediate factors, such as loyalty to a party label—will play a role here, perhaps sustaining loyalty out of all proportion to actual material benefits. Sacks, in describing Ireland's "Donegal Mafia," shows that political culture sustained much of the mass support for a machine whose rewards were mostly "imaginary patronage" (Sacks, 1976, Chs. 1–3). The intangible, perceptual dimensions of this seemingly most materialistic kind of politics need further understanding.

In the end, our analysis of patronage politics needs to be both more specific and more general: more specific, in the sense that we must be aware of all the complications identified here, and the variations they produce from case to case; and more general, to develop a systematic understanding of how such factors as the degree of divisibility of rewards, qualifications on the reciprocity principle, and the aging of organizations function in diverse cases. Such a "general theory of patronage" could be a powerful tool in comparative analysis. It could help us understand parties and movements "from the inside out"—as the products of actual behavior and choices, not of impersonal social forces.

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Presidential Control of the Senior Civil Service: Assessing the Strategies of the Nixon Years*

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Modern presidents must be attentive to influences of the federal bureaucracy on their policy initiatives and all attempt some measure of bureaucratic control. This article assesses the extent of President Nixon's success in gaining some degree of management control over the bureaucracy through the manipulation of the civil service personnel system. We find that Republicans were, in fact, more likely to be selected to top career positions during the Nixon years. We find also that career executives calling themselves Independents were more likely during the Nixon years than before to resemble Republican executives in their support of Nixon's policies and goals. This is significant to presidential control because of the large number of bureaucrats calling themselves Independents. We conclude that Independent career executives may provide a president with a considerable reservoir of bureaucratic support.

Contemporary American presidents have often expressed suspicion, frustration, and dismay in dealing with a bureaucracy which may seem aloof, unconcerned, and at times even hostile to them and their policies. President Franklin D. Roosevelt, in commenting on bureaucratic recalcitrance, once said, "The Treasury and the State Departments put together are nothing compared with the Navy. To change anything in the Navy is like punching a feather bed. You punch it with your right and you punch it with your left until you are finally exhausted, and then you find the damn bed just as it was before you started punching" (Neustadt, 1960, p. 42). President Kennedy once described the State Department as a "bowl of jelly. It's got all those people over there who are constantly smiling. I think we need to smile less and be tougher" (Schlesinger, 1965, p. 406).

Of course, the contemporary president, in attempting to shape and direct public policy, cannot ignore the bureaucracy. The classic model of a neutral bureaucracy responsible only for deciding the best means for implementing policy whose goals and objectives have been established by elected political officials has long been replaced by one recognizing, as Hugh Heclo says (1977, p. 21), the role of the civil servant as "an unexpected and insecurely placed [full] participant to the original grand design of American government." Francis Rourke (1969) has impressively documented the impact the bureaucracy can have on public policy because of its ability to mobilize political support and to apply or deny bureaucratic expertise. Rourke (1969, pp. 50–51) finds that "no aspect of the growth of bureaucratic power in this country has been more important than the steady expansion in the scope of administrative discretion. . . . Today all bureaucratic decisions [have] some implications for policy."

Modern presidents, then, must be attentive to bureaucratic influences on policy directions and it is clear that all recent presidents have attempted some measure of bureaucratic control and direction. The literature on this aspect of the presidency (Koenig, 1968; Rourke, 1969; Thomas, 1970) usually distinguishes between the more formal and hierarchical approach utilized by President Eisenhower with the less structured, more open and competitive approach used by a Roosevelt or a Kennedy.

As the literature on the Nixon presidency grows in volume, it is becoming evident that Nixon's struggle with the bureaucracy was more intense, more calculated, and far more political in design than that of any previous president. Heclo (1977, p. 75) has concluded that "old-

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time government officials might have thought that they knew that management meant business-like administration, but [under Nixon] they were in for an awakening." Richard Nathan, himself an early member of Nixon's staff, states (1975, pp. 7–8) that with the full deployment of the Nixon strategy, "no longer would the Cabinet be composed of men with national standing in their own right. . . . The President's men—trusted lieutenants, tied closely to Richard Nixon and without national reputations of their own—were to be placed in direct charge of the major program bureaucracies of domestic government. . . ."

This article examines one of the means by which the Nixon White House attempted to gain such a high degree of management control over the bureaucracy—the manipulation of the civil service personnel system. This article's *principal goal is to document the extent to which the administration actually succeeded in its design.*[1] How did the Nixon White House propose to assume, in the words of Nathan (1975), "control of the machinery of domestic government" and to what degree was the Nixon administration able to change the partisan nature and issue positions of the senior-most levels of the career civil service through its personnel policies? These are important questions for an understanding of domestic policy making and implementation in the Nixon years (and by implication, for future presidents).

President Nixon and the Federal Bureaucracy

The Nixon White House held conflicting views of the federal bureaucracy. On the one hand, the bureaucracy was seen as a major (often hostile) impediment; on the other hand, it was seen as a potentially powerful political resource. As an impediment, the bureaucracy was believed by the Nixon White House to be staffed at the senior-most levels with personnel remaining loyal and dedicated to the programs and goals of the previous Democratic presidents. Nixon assumed the presidency at a time when the federal bureaucracy was experiencing substantial growth. In the early 1960s, the number of federal employees was 2.4 million; by 1970 the number had increased to about 3 million. Furthermore, the departments and agencies concerned with domestic social issues recorded the most rapid staff and budgetary growth rates during this period. The Department of Housing and Urban Development (including its pre-cabinet status) grew by 37 percent and Health, Education and Welfare expanded by 75 percent. In addition, the federal budget grew from approximately $92 billion in 1960 to nearly $200 billion by 1970. While the proportion of the federal budget allocated to defense decreased during the sixties, the proportion (*Statistical Abstract,* pp. 236, 306, 221) allocated in such areas as health and education continued to rise. Thus, the Democratic administrations preceding the Nixon years were characterized by growth in the federal bureaucracy and expansion in budgetary outlays, and this growth was most noticeable in domestic social service programs.

Possibly influenced by these developments, one Nixon aide indicated in 1969 (Nathan, p. 82) that "the White House was surrounded" by powerful bureaucratic interests opposed to the president's program. President Nixon himself (1971a, p. 167) referred to his opponents in the bureaucracy as "dug-in establishmentarians fighting for the status quo," and stated further that, "I think it is repugnant to the American system that only the bureaucratic elite at the top of the heap in Washington [believes it] knows what is best for the people. . ." (Nixon, 1971b, p. 463).

These tensions between the White House and the bureaucracy were intensified as many of Nixon's new federalism proposals called for a reduction in the number of categorical grants, the consolidation of various departments and agencies, and the elimination of federal bureaucratic discretion in various grant-in-aid programs. Not only was the bureaucracy viewed as consisting largely of personnel selected during and remaining loyal to previous administrations, but also many of Nixon's domestic programs could be interpreted as posing direct threats to the careers of many federal executives. It is not surprising that the White House anticipated negative reactions from the senior bureaucracy and, indeed, Joel Aberbach and Bert Rockman (1976) have shown that in 1970 a sizeable proportion of the senior bureaucratic corps was ideologically and politically opposed to the Republican White House.

The Nixon White House, on the other hand, also viewed the bureaucracy as a potential source of power and influence. In his first term, Nixon had been largely unsuccessful in generating much support within Congress for his domestic program—a program he equated to a "New American Revolution." In Nixon's first term his general revenue sharing program had died in Congress (although, of course, the program did eventually pass), the Congress refused to consider his massive departmental reorganization scheme, none of his special revenue sharing programs appeared close to passage, and his major welfare reform proposal had not been reported out of the Congress. Frustrated by Congress' early reluctance to act swiftly and positively on his major domestic legislative programs, Nixon sought to maximize managerial control over the bureaucracy and both isolate it from Congress and make it more responsive to the White House. Thus began a concerted strategy of placing top aides and loyalists in key civil service positions. Richard Nathan (1969, pp. 8, 61–62) best describes the process when he says that following Nixon's first term the "traditional legislative strategy" of governing was abandoned and a "fundamentally different approach" was adopted. The new strategy, according to Nathan, would be designed "to *take over* the bureaucracy and *take on* Congress." This would be accomplished, says Nathan, by placing Nixon's "own trusted appointees in positions to manage directly key elements of the bureaucracy. . . . The new appointees would be the President's men. The bureaucracy would report to them; they would be held accountable."

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[1] Of course, personnel management is but one tactic a president may employ in attempting to gain some measure of bureaucratic control. Nixon, himself, also attempted to impose extensive reorganization programs which also would have brought greater presidential management. We focus here, however, on personnel management—the dominant theme of Nixon's management efforts.

Nathan (1969, pp. 59–95) labeled these bureaucratic strategies the "administrative presidency"; Heclo (1977, p. 75) calls it "management control." Regardless of labels, the design was to achieve such control over the top policy-making positions in the bureaucracy that policy and program objectives denied the White House by the legislative process could be achieved through bureaucratic decision making.

These two views of the bureaucracy, often confusing and contradictory, were paramount in shaping Nixon's strategy of dealing with the bureaucracy. On the one hand the Nixon White House viewed senior career executives as, on the whole, disloyal. On the other hand, the bureaucracy was seen as a possible alternative for achieving at least some of the domestic objectives which Congress appeared unwilling to approve. Redirecting the basic values and the partisan orientations of the senior levels of the bureaucracy became a major focus of the Nixon White House and the major political strategy used to pursue the administration's domestic policies. This, indeed, did represent a fundamental shift in bureaucratic-presidential relations and requires careful examination both in terms of its impact on the federal bureaucracy and on domestic policy making in general.

Data and Methodology

Data for this analysis are drawn from several sources, but primarily from a larger survey of top federal officials conducted by the authors in 1976. Since we are concerned with assessing the role of the bureaucracy in domestic policy making—most specifically, "new federalism"—our sample is drawn from the federal departments and agencies most concerned with domestic affairs, thus excluding administrators from the Defense and State Departments.[2] To be eligible for the sample, officials in these domestic agencies had to occupy positions with full-time policy and program responsibility (eliminating those concerned only with internal duties such as personnel, employee safety, information matters, and the like). From a list supplied by the U.S. Civil Service Commission indicating the names, titles, and positions of every supergrade official, we identified the top political and career executives within each agency. These were those specified as political executives or career civil servants holding "supergrade" status (GS 16–18, or the equivalent). Of the 524 so identified, 195 completed and returned our survey. Table 1 presents the agency by agency response rate—ranging from a high of 50 percent from HUD to a low of 24 percent from Treasury—and also examines the representativeness of the sample. As Table 1 indicates, Agriculture and HUD are slightly over-represented and Treasury is slightly under-represented but, overall, the response sample is quite representative of the totality. Additional measures of sample representativeness are discussed below.

Much of the following analysis will compare political appointees with career executives at the supergrade level. However, the distinction between political and career executives is not precise and experts often disagree as to the boundaries defining each. According to Heclo (1977, pp. 36–55), the proportion of top executives classified as political appointees can range from 9 to 25 percent depending on how various positions are defined and classified.

Following Aberbach and Rockman, we classified all positions as either career executives or political appointees. Schedule A, B, and C positions are considered political appointees for purposes of this study as are all Noncareer Executive Assignments (NEAs). This classification follows the usually accepted nature of these appointments. Since we are specifically interested in the extent to which the attitudes and characteristics of those federal bureaucrats selected during the Nixon administration differ from those selected during prior administrations, the following analysis often will divide the sample accordingly.

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[2] We received replies from each of the following departments and agencies included in our study: Agriculture, Commerce, HEW, HUD, Interior, Justice, Labor, Transportation, Treasury, GSA, FCC, FEA, FPC, FTC, ICC, SEC, SBA, VA, EPA.

The Aberbach and Rockman study (1976) conducted shortly after Nixon's first term began provides an important comparative perspective for this study. Students of the federal bureaucracy will recognize our sample as closely paralleling theirs. In fact, our pool of departments and agencies is identical to theirs except that OEO had been disbanded by 1976 and the Federal Energy Administration and the Environmental Protection Agency had recently been established. These changes, of course, are reflected in our sample.[3]

Aberbach and Rockman were concerned with questions of general political and issue orientations (such as attitudes toward more or less government service), while our survey probed the attitudes of those senior officials to a number of specific domestic programs proposed and supported by President Nixon. Still, the two surveys provide an excellent opportunity to make "before and after" inferences about the Nixon bureaucracy. The Aberbach and Rockman study indicated the general orientations of the top bureaucrats during the first two years of the Nixon administration, while our data permit an examination of attitudes toward a number of specific issues at the conclusion of the Nixon years. If the Heclo/Nathan thesis is correct—that Nixon was determined to achieve a degree of bureaucratic politicization—this should be reflected in these comparisons.

We are not suggesting that Aberbach's and Rockman's measure of general orientations toward the provision of government services is equivalent to our measures of attitudes toward specific policies and programs, but we do believe that useful comparisons are possible. To the extent that philosophical differences exist between a president and the bureaucracy, these differences become politically important when considered in the context of specific programs and policies. A president has little to fear from a bureaucracy ideologically opposed to the president's philosophy yet willing to accommodate without reservation policy directives in pursuit of that philosophy. If we demonstrate that such accommodation has taken place either within the population of supergrade officials as a whole or some subpopulation of that group, we believe this has political significance. Still, we readily acknowledge that we cannot demonstrate in this study whether

Table 1. Sample and Response Rate

Department	Percent of Universe	Response Rate	Percent of Sample
Agriculture	8%	46%	10%
Commerce	8	40	9
HEW	13	33	12
HUD	8	50	12
Interior	13	34	12
Justice	7	31	6
Labor	4	41	5
Transportation	4	27	3
Treasury	6	24	4
All Others	28	33	27
Totals	99%		100%

Source: Data compiled from authors' survey of senior federal officials conducted in 1976.

Note: Here and throughout this study percentages may not total 100% because of rounding.

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[3] The Aberbach and Rockman study is also useful in assessing the representativeness of our sample. Aberbach and Rockman limited their sample to 144 possible respondents and interviewed 126. Our universe was larger since we attempted to include everyone meeting the above described sample requirements. Our return rate was lower than their rate of completed interviews; however, our response samples are similar in many respects. For example, Aberbach and Rockman found that 17 percent of the career executives in their sample identified with the Republican party; 16 percent in our sample identified with the Republicans. Sixty-six percent of the political appointees in the Aberbach and Rockman sample identified with the Republican party, as opposed to 61 percent in our sample. Twenty-nine percent of all those in the Aberbach and Rockman study were associated with a social service agency (HEW, HUD, OEO), while 23 percent of our sample were associated with either HEW or HUD. Since our studies are separated by six years, some variance is expected; yet, in those areas where comparisons are possible, the response samples are quite similar. Another indication of the representativeness of our sample is that 29 percent of our respondents are classified as political appointees, a figure which deviates from the population by only 4 percent according to Heclo when he employed the classification scheme used in this study.

changes have taken place in the general attitudes of senior executives toward government provision of services.

Reshaping the Bureaucracy

Seeking out and placing in key leadership positions individuals philosophically compatible with and loyal to Nixon became the stated objectives of the White House personnel organization. A management study prepared by the White House (Subcommittee on Manpower and Civil Service, 1976, p. 411) stated that "management control could be achieved by attracting the best qualified individuals who are philosophically compatible and loyal to the President, placing them in leadership positions, motivating them by recognizing and promoting outstanding performance, and removing any whose performance is poor. At the same time, personnel decisions should be made and announced to maximize political benefit and minimize political loss." The question arises as to how was it that the White House hoped to successfully influence the selection of top officials—a process supposedly protected from such incursions by a host of civil service rules and regulations. Obviously, the president was expected to name his own appointees to important political positions. However, the essence of the administrative presidency argument is that White House influence was to extend to the career civil service as well. How was this to be accomplished?

Shortly after Nixon's first term began, the White House Personnel Operation (WHPO), headed by Harry Flemming, was established to identify and place individuals in presidential appointment positions. Following initial White House dissatisfaction with Flemming's direction of the personnel operation, Frederick Malek (then HEW Deputy Under-Secretary) was asked to study the Flemming operation and to prepare a comprehensive plan. Ultimately the White House Personnel Manual (Subcommittee on Manpower and Civil Service, 1976, pp. 573–686) was produced and distributed in November, 1972 as a guide to executives in the hiring and placement of personnel.

The first step, according to the manual (Subcommittee on Manpower, 1976, p. 163), was to identify vacancies. When no vacancies existed, some would have to be created and the manual provided specific guidelines for this activity:

> There are several techniques which can be designed, carefully, to skirt around the [difficult problem of firing established career executives]. You simply call an individual in and tell him he is no longer wanted . . . you expect him to immediately relinquish his duties. There should be no witnesses in the room at the time.

If this "frontal assault" (the manual's own terminology) were not successful, the manual went on to suggest transferring unwanted personnel to regional offices, described by the manual as "dumping grounds." "If you have an employee," the manual advised, "who was born and raised in New England and is currently serving in your Boston regional office, and his record shows reluctance to move far from that location, a transfer accompanied by a promotion to an existing or newly created position in Dallas might just fill the bill." If the transfer technique failed, the manual recommended other "special assignments" as well as other techniques. But the point was clear: where no positions at the senior career levels were open, political executives were instructed in the art of creating such openings by encouraging the firing or transferring of personnel.

In addition, once an open position was identified, the White House wanted to ensure that "its" personnel were appointed to that position. Malek (Subcommittee on Manpower, 1976, p. 166) described this process in Senate testimony as follows:

> What we are doing in the case of a career position is we would be submitting the name of a person to a department and asking them to determine where this person would be qualified and competitive, to serve in that position, to try to get them into it.
>
> So, what we were really doing is facilitating the personnel process in getting somebody in that door, where, without the political push, they may not have been getting into the door.

Thus, through the process of first identifying vacancies (or "encouraging" senior personnel to vacate) and secondly by ensuring that candidates supportive of the administration's position were referred to appropriate political personnel contacts in the departments and agencies, the White House expected to maximize the placement of "its people" in senior career positions. In this manner, the Nixon administration hoped to circumvent the "normal" civil service process and to achieve a high degree of managerial control over the federal bureaucracy. The following sections measure the extent to which the White House succeeded in achieving these goals.

The Federal Bureaucracy: The Changing Nature of Party Identification

As Richard Nixon began his first term, he faced a bureaucracy whose personnel and character had largely been shaped in the Kennedy/Johnson years. Nathan (1975, p. 49) argues that Nixon's staff *initially* recommended personnel for top civil service and appointive positions on the basis of "ability first, loyalty second." This began to change in earnest in the fall of 1970 when Nixon's aides began their concerted attempts to alter the bureaucracy. As recounted by Heclo, the Civil Service Commission found itself increasingly subject to White House pressure during this period. Quoting from an Executive Order of July 1, 1970, Heclo (1977, p. 27) finds that the Bureau of the Budget (later changed to the Office of Management and Budget) was "charged with advising the President on the development of new programs to recruit, train, motivate, deploy and evaluate the men and women who make up the top ranks of the civil service."

An important empirical question, then, is the extent to which the Nixon White House was able to affect the partisan affiliation of the top levels of the federal bureaucracy. In order to answer this, of course, we need data for both the early and late Nixon years. Here, as elsewhere in this article, we compare the 1970 Aberbach and Rockman survey with our 1976 data. Table 2 presents this comparison for partisan affiliation.

As Aberbach and Rockman have concluded, few Republicans (17 percent) were found among those holding top career executive positions in the early Nixon years (see Table 2). The 1970 differences are even more striking when one recalls that the "Independents" in the Aberbach and Rockman survey were more likely to resemble Democrats than Republicans. Typically, about two-thirds of these Independents were found by Aberbach and Rockman to side with Democrats when answering questions of general policy orientations. If these Independents had been forced to select one party (Aberbach and Rockman did not attempt this), a generous assessment of the proportion of top career executives who in 1970 identified with the Republican Party would be only about one-third (assigning half of the Independents to each party).[4]

Considering the 1976 data presented in Table 2, the most noticeable change one finds over this period is in the decline of top executives (both political and career) identifying with the Democratic Party and the increase in the proportion calling themselves Independents. The proportion of Republican executives remained about the same. At the very least, one might conclude that after eight years of a Republican administration, Democratic identifiers were less likely to reveal their partisan choice.

Table 2. Party Affiliation and Job Status, 1970–1976

	Party Affiliation										
	1970				1976				1976[b]		
Job Description	Dem.	Ind.	Rep.	(N)	Dem.	Ind.	Rep.	(N)	Dem.	Rep.	(N)
Political Appointees[a]	24%	10%	66%	57	19%	19%	61%	57	25%	75%	55
Supergrade Career[a]	47	36	17	58	38	46	16	111	60	40	100
Totals:				115				168			155
Gammas:		–.58				–.59			–.63		

Source: Joel Aberbach and Bert Rockman, "Clashing Beliefs within the Executive Branch," *American Political Science Review* 70: 459, and authors' survey of senior federal officials conducted in 1976.

[a] Percentages add row-wise.

[b] Data represent "forced" party choice. Independents were asked to indicate for which party they usually voted.

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[4] Caution must be exercised when interpreting the Aberbach and Rockman data in this fashion. Aberbach and Rockman recorded their respondents' voting behavior rather than their actual party identification, as we asked. Further, noting that Independents are more likely to resemble Democrats in general policy orientations (as do Aberbach and Rockman) is not necessarily the same as finding Independents more likely to vote Democratic at the ballot box. Still, we believe that these items are similar enough to ours to suggest *possible* inferences of partisan choice. At the same time, we realize that when dealing with slightly different questions, one incurs certain unavoidable risks. Perhaps future research may clarify these distinctions.

In order to determine the partisan leanings of the Independents, this study forced a party selection by asking Independents to indicate for which party they usually voted. We then found significant distinctions, as noted in Table 2. Slightly more than 50 percent of both political and career executives who indicated that they were Independents stated a preference for the Republican Party at the ballot box. The proportion of Independents who by 1976 sided with the Republican perspective was clearly greater than that found by Aberbach and Rockman in 1970. Examining the "forced choice" partisan distribution one can see that in 1976, 75 percent of the political appointees and 40 percent of the career executives either were Republicans or voted Republican.

This represents a significant increase over 1970 and indicates that although the White House's strategy may not have succeeded in markedly increasing the number of Republican Party members at the most senior levels of the civil service, it did result in the selection of a high proportion of Independents who voted Republican or, perhaps, in influencing some Independent identifiers to lean in a Republican direction.

By dividing the sample into time frames representing the date of an executive's appointment, we can gain greater insight into the effects of the administrative presidency. Table 3 divides, by job status, the 1976 sample into those selected prior to the Nixon administration and those selected during the Nixon years.[5]

Table 3 reveals two points of interest. First, Democrats were considerably less likely and Republicans and Independents considerably more likely to be selected for senior political and career executive positions in the Nixon years than before. The proportional decline in Democrats selected for career positions (from 40 to 24 percent) seems particularly significant. Obviously, affiliation with the Republican party was a greater asset and affiliation with the Democrats a greater liability to job aspirants during the Nixon era than before.

Second, Table 3 demonstrates the limits of a strategy designed to affect major change at the top levels of the federal bureaucracy in a short period. Of the total 1976 sample of top executives, almost 70 percent assumed their positions before the Nixon administration began. Considering only career executives (to which the strategies described above were really directed), almost 85 percent had assumed their positions in the pre-Nixon years. Thus, in the short time span of a single presidency the

Table 3. The Changing Nature of Party Affiliation by Job Status

Party Identification	Political Appointees: Pre-Nixon	Political Appointees: Post 1968	Career Executives: Pre-Nixon	Career Executives: Post 1968
Democrat	26%	13%	40%	24%
Independent	22	17	45	53
Republican	52	70	15	24
Total:	100%	100%	100%	101%
N:	27	30	94	17
Gamma:	.34		.31	

Source: Data compiled from authors' survey of senior federal officials conducted in 1976.

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[5] A methodological problem particularly relevant to this study is the problem of inferring longitudinal representativeness of cohort samples based on data collected at one point in time. Even though our sample fairly accurately represents the 1976 population of senior bureaucrats, there is no way of determining whether time cohorts generated from this sample necessarily represent the attitudes and characteristics of all those in office during the particular time intervals selected for analysis. Employees come and go, and it might be argued that those who were most favorable to the goals and objectives of new federalism and Nixon's domestic policies were more likely to remain with their particular agencies throughout the years. While we realize the seriousness of this problem, two factors mitigate its impact on this study. First, our study will show that the cohorts generated do differ significantly in the attitudes and characteristics to be examined. To the extent that self-selection has taken place and that some officials critical of new federalism issues resigned in protest, *our arguments actually are strengthened.* More important, the Aberbach and Rockman study provides an essentially identical sample frame drawn from an earlier period. These data do allow for "before and after" comparisons and we make use of their sample whenever possible.

proportion of appointments available compared with the total number of top federal bureaucrats is relatively small. Even if every career vacancy had been filled during the Nixon years by a Republican, the numerical impact would have been marginal.[6] This would suggest that truly effective presidential management of the bureaucracy must combine elements of control over the selection process with influence over bureaucrats already holding top policy positions.

Party Affiliation by Agency

A question of additional interest concerns the actual placement of personnel. As demonstrated above, those agencies experiencing greatest expansion in the decade of the 1960s were those usually defined as social service-oriented, i.e., HEW, HUD, and OEO. It is no secret that the clientele of these agencies were not viewed as particularly central to the Nixon base of political power. The conflicts resulting in Nixon's attempt to dismantle OEO attest to the mutual bitterness and disdain between the White House and some agency officials. Thus, the administrative presidency was designed also to correct what was considered to be an imbalance in the partisan leanings of top officials in the social service agencies. Table 4 examines the extent to which the partisan affiliation of senior officials in social service and non-social service agencies was affected during the Nixon years.

Table 4 reveals a noticeable shift in the partisan composition of these domestic agencies between 1970 and 1976. Relying once again on the Aberbach and Rockman 1970 survey, we can see that Republicans comprised only 24 percent of the top bureaucratic positions in the social service agencies (HEW, HUD, OEO) as the Nixon administration began. Aberbach and Rockman found that 46 percent of these top social service positions were staffed by Democrats, 30 percent by Independents. Since Aberbach and Rockman found Independents to resemble Democrats more frequently, a safe estimation of the actual proportion of executives supporting or leaning toward the Republican party would have been around one-third.

By 1976 we observe a pattern somewhat similar to that identified above when we examined the changing partisanship of political and career executives of this period (Table 2). That is, a lower proportion of Democrats was found among the top service positions in 1976 (from 46 to 33 percent) and the ranks of Republicans had increased somewhat (24 to 30 percent). As was the case when we examined job status, an important shift also is evident in the proportionate number of executives calling themselves Independents. Thirty-seven percent of those top executives in the social service agencies considered themselves Independents in 1976 (compared with 30 percent in 1970) and 38 percent of those executives in non-social service agencies considered themselves Independents (compared with 21 percent in 1970). Also, as we found when considering job status, the most interesting relationship over this period is revealed when we asked questions forcing these Independents to make a partisan choice. What appeared to be a slight Republican increase in social service executives (from 24 to 30 percent) was, in reality, rather large (from 24 to 44 percent) and what appeared to be a

Table 4. Partisan Affiliation by Agency, 1970–1976

	Agency					
	1970		1976		1976[a]	
Party Identification	Social Service	Other	Social Service	Other	Social Service	Other
Democrat	46%	33%	33%	32%	56%	46%
Independent	30	21	37	38		
Republican	24	46	30	30	44	54
Totals:	100%	100%	100%	100%	100%	100%
N:	33	82	43	141	36	132
Gammas:	.32		.002		.19	

Source: Joel Aberbach and Bert Rockman, "Clashing Beliefs within the Executive Branch," *American Political Science Review* 70:459, and authors' survey of senior federal officials conducted in 1976.

[a] Data represent "forced" party choice. See text for explanation.

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[6] We are grateful to Hugh Heclo for this important observation.

drop in Republican executives in non-social service agencies (from 46 to 30 percent) actually is an increase (from 46 to 54 percent). Forcing such a selection obviously increases the proportion of Democrats in these various agencies as well, but Republicans still maintained their advantage over Democrats in non-social service agencies, as noted by Aberbach and Rockman in 1970, and their proportion serving in the social service agencies noticeably increased.

It has been shown that proportionally more Republicans were selected for senior political and career positions during the Nixon years than before (Table 3) and that social service personnel were more likely in 1976 than in 1970 to identify with the Republican Party (Table 4). It remains to be discovered whether the increasing Republican composition of the social service agencies is reflective only of the president's political appointments, or whether significant partisan change among career executives is also evident. In other words, is the apparent shift in Republican identifiers among social service personnel accounted for by political appointees only; or were those trends indicative of more basic partisan changes among the career service personnel as well? Table 5 examines this important question.

As indicated in Table 5, the sample sizes, in refining the analysis in this manner, sometimes become quite small. Only four political appointees selected prior to 1968 serving in the social service agencies were represented in this survey (and only one of these was a Democrat). This information, then, must be considered with some degree of caution; still, interesting trends are evident.

It can be seen that for all categories of agency affiliation and appointment status, Democrats were proportionally *less* likely to be selected in the Nixon era than before. This is especially true (as would be expected) of political appointees, but we also found that *career executives* identifying with the Democratic party selected to social service agencies and all other departments were proportionally fewer during the Nixon administration. The decline in Democratic identifiers corresponds with a proportionate increase in both Independents and Republicans. While small sample sizes here dictate extreme caution, the data indicate that Republican career executives were about 3 times as likely to be promoted to senior positions in the social service agencies during the Nixon years and more than 1.5 times as likely to be promoted to senior positions in all other departments and agencies. Table 5 demonstrates that it is also true that Democrats were not totally excluded from these senior-level positions during the Nixon years. While their proportionate numbers clearly declined, some Democrats succeeded in achieving high political and career positions during the Nixon years. Thus, while partisanship was not an exclusive factor in the selection of senior level positions during the Nixon years, it obviously was one important consideration. Republicans were more likely to be selected for all these categories of appointed and career social service and "other" positions during the Nixon years than before.

In support of Heclo and Nathan, then, we have found that the "administrative presidency" was having a noticeable impact in strengthening the proportionate number of Republicans selected not only for political positions (as would be expected) but also for high-level career senior civil service positions. Proportionally more Republicans were selected for more career executive positions during the Nixon years than before and, through this process, the White House was beginning to achieve partisan equality in the key social service agencies. Additionally, by the selection of Independents who voted Republican or by

Table 5. Partisan Affiliation by Appointment Authority, Agency, and Appointment Date

	Pre-Nixon				Post 1968			
	Agency				Agency			
	Social Service		Other		Social Service		Other	
Party Identification	Political	Career	Political	Career	Political	Career	Political	Career
Democrat	25%	52%	26%	39%	8%	33%	17%	18%
Independent	25	41	22	45	25	50	11	55
Republican	50	6	52	16	67	16	72	27
Totals:	100%	99%	100%	100%	100%	99%	100%	100%
N:	4	17	23	75	12	6	18	11

Source: Data compiled from authors' survey of senior federal officials conducted in 1976.

influencing some Independents to lean in a Republican direction (or through some combination of these), the administrative presidency was to achieve among Independent bureaucrats a level of support for the Republican party much greater than before.

Partisan affiliation is one key barometer of the changes taking place during the administrative presidency. Just as important are the actual attitudes of those selected for political and career positions. To what extent did those selected during the Nixon years differ in their attitudes toward key Nixon proposals from those selected before the Nixon presidency? This question is considered in the following section.

The Federal Bureaucracy: Attitudes Toward New Federalism

Aberbach and Rockman (1976, p. 461) examined bureaucratic attitudes toward government provision of social services. After constructing a five-point continuum ranging from favoring much more to favoring much less government provision of social services, Aberbach and Rockman found that 44 percent of their sample favored such provision and 26 percent opposed. This finding, they argued, at least partially confirms suspicions by the Nixon White House that the orientation of senior career officials was biased against a more conservative approach.

By 1976, of course, the domestic policies and goals of the Nixon administration had been clearly articulated and, for the present study, it is appropriate to consider questions dealing with bureaucratic attitudes toward these specific programs. Whereas Aberbach and Rockman focused on general orientations, these 1976 respondents were asked to indicate their attitudes concerning a number of specific program and policy goals of Nixon's domestic proposals. Each of these questions dealt with some aspect of Nixon's new federalism program—a program designed to consolidate a number of categorical grants and return decision making to state and local officials. As in the Aberbach and Rockman study, officials were asked to record their responses along a five-point continuum. Thus, we are able to examine more directly bureaucratic attitudes toward the president's specific policy program and objectives.[7] Table 6 examines the marginal distribution of responses to the four new federalism programs which actually were enacted by the Congress and to four key policy objectives of the new federalism philosophy.

The most obvious conclusion to be drawn from Table 6 is that by 1976 considerable support for Nixon's new federalism programs and objectives existed among top political and career federal executives. For each program examined, over 60 percent of those surveyed expressed positive support. Very little variance in overall response is noted except that a slightly higher proportion expressed negative attitudes toward the general revenue sharing program than to the programs of special revenue sharing.

When we examine party identification, agency affiliation, and job status, we notice additional variances which generally are in the direction predicted by the Aberbach and Rockman study. With the exception of ease of the grant application procedures, strongly endorsed by all groups, Republican bureaucrats were consistently more favorable to both the programs and policy goals of new federalism than were Democrats. Again, it is very important to note that Independents generally were more likely to resemble Republicans in their responses to these questions—especially those questions dealing with specific programs. This is a significant change from 1970 when, it will be recalled, Aberbach and Rockman found Independents generally siding with Democrats.

Agency affiliation made surprisingly little difference in response patterns, but we found job status to have a considerable effect on attitudes toward new federalism programs and goals. Political appointees indicated quite favorable attitudes; career executives were more mixed.

Perhaps the most important conclusion to be reached from these data is that by 1976 top political and career executives were on the whole quite supportive of Nixon's new federalism programs and goals. Over a majority of all executives, regardless of party identification, agency affiliation, or job status supported every program and policy we examined.

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[7] We acknowledge the problems of measuring attitudes about a president's program after the president has left office. By 1976 Nixon's domestic programs were gaining wider acceptance in the Congress, among state and local officials, and among the public as a whole. It is not surprising that the bureaucracy, too, would be more positive in its evaluation. However, it is not so much the absolute level of acceptance or rejection of the president's program which concerns us here. We are more concerned with the *relative* level of acceptance among the various subpopulations examined. To the extent that differences in subgroups are found, the central issue posed by this study—the attitudinal differences between those senior executives selected prior to and during the Nixon years—can be explored.

The Changing Nature of Bureaucratic Attitudes

The central theme of proponents of the administrative presidency is that the Nixon

Table 6. Attitudes Toward New Federalism Programs and Policies

	Party Identification				Agency Affiliation		Job Status	
	All 195	Dem. 59	Ind. 71	Rep. 56	Social Service 47	Others 145	Political 57	Career 116
A. Attitude Toward Program of:								
1. General Revenue Sharing								
Positive	62%	54%	65%	71%	61%	63%	77%	56%
Neutral	14	10	16	15	10	15	9	15
Negative	24	36	19	15	29	22	14	29
2. Housing & Community Development (CDBG)								
Positive	63	55	65	66	69	61	68	61
Neutral	18	16	23	14	16	21	18	18
Negative	19	29	12	20	16	19	14	21
3. Law Enforcement (LEAA)								
Positive	67	54	70	76	57	70	71	66
Neutral	14	16	15	13	19	13	13	14
Negative	19	30	15	11	24	17	16	20
4. Manpower (CETA)								
Positive	61	53	61	68	61	61	74	55
Neutral	18	14	26	14	21	18	16	19
Negative	21	33	13	18	18	21	11	27
B. Attitude Toward Policy of:								
1. Formula Distribution								
Positive	79	71	79	88	85	78	97	73
Neutral	6	5	9	5	2	7	2	7
Negative	14	23	12	7	13	15	2	20
2. Return of Decision Making to States and Localities								
Positive	78	68	77	89	76	79	90	74
Neutral	4	5	4	2	4	4	4	4
Negative	18	26	19	9	20	18	7	23
3. Ease of Application								
Positive	94	97	91	93	94	92	95	93
Neutral	4	3	4	5	4	4	5	4
Negative	2	0	4	1	2	3	0	4
4. New Federalism Philosophy as an Improvement over Categorical Grants								
Positive	65	54	64	75	71	61	76	60
Neutral	14	14	16	11	2	17	12	13
Negative	21	33	20	15	27	22	12	27

Source: Data compiled from authors' survey of senior federal officials conducted in 1976.

Note: In responding to these items, the executives were presented the list of program and policy goals and asked to indicate their feelings about each along a five-point continuum ranging from "very positive" to "very negative." For the final question, respondents were asked to indicate along a continuum of "strongly agree" to "strongly disagree" their feelings as to whether or not the philosophies reflected in Nixon's new federalism proposals represented improvements over the categorical grant system. In this table the two positive and the two negative responses for all items are collapsed into a single "positive" or "negative" response for clarification and ease of presentation. Because of missing data, subcategory totals vary and are somewhat lower than the total N. Of course LEAA was first established before the Nixon administration; however, the program grew substantially during the Nixon years and often was called by Nixon his "model" for his own special revenue-sharing proposals.

White House was able to influence placement of people sympathetic to Nixon's domestic policy proposals in important civil service positions. It has already been shown that a higher proportion of Republicans were selected for these positions in the Nixon years and that Republican executives in general were more sympathetic to the policies and goals of new federalism. This section focuses on the question of *changing* bureaucratic attitudes over time. If, as Nathan and Heclo have argued, the Nixon White House extended the politicization of the bureaucracy further than any previous administration, it would be expected that the attitudes of bureaucrats placed during the Nixon years would deviate from those receiving their appointments earlier.[8] Table 7 summarizes these findings.

Table 7 generally confirms these expectations. Considering first the entire sample of senior officials (political appointees as well as career executives), in all cases those selected during the Nixon years are more positive toward those programs and policy issues of Nixon's new federalism than those selected in previous years. Nathan (1975, pp. 59–29) argues that in the later Nixon years loyalty really became the dominant factor in promotions and appointments. While it is not shown in Table 7, we found later Nixon appointees to be more positive even than those selected early in the administration.

It also is true, as shown in Table 7, that these differences were found among *both* political and career executives. While political appointees are expected to reflect the philosophy of the current administration, the essence of the Heclo/Nathan argument is that the Nixon White House extended this to include career executives as well. While the data may not be overwhelming (in four instances the differences are not found to be statistically significant), it is true that in all instances, with the exception of the CETA program, career executives selected during the Nixon years were more sympathetic to the president's program than those selected during previous years. Sometimes, these differences were rather large. For example, of those career executives selected during the Nixon years, 71 percent and 75 percent expressed positive attitudes toward the General Revenue Sharing and Community Development Block Grant programs, respectively. By contrast, only about half of those career officials selected prior to the Nixon years indicated positive attitudes toward these programs (not shown in Table 7). Indeed, those advanced to top career civil service positions in the Nixon years were on a whole more positive toward Nixon's own programs and philosophies than were those selected in previous years.

Table 7. Mean Differences Between Pre- and Post-Nixon Appointees

	Entire Sample			Career Executives Only		
Attitudes Toward:	Pre-Nixon	Post-Nixon	Sig.	Pre-Nixon	Post-Nixon	Sig.
Programs						
General Revenue Sharing	2.6	2.1	.01	2.7	2.2	.05
Housing and Community Development (CDBG)	2.4	2.2	NS	2.5	1.8	.01
Law Enforcement (LEAA)	2.5	2.2	.05	2.5	2.1	NS
Manpower (CETA)	2.5	2.4	NS	2.6	2.6	NS
Policies						
Formula Distribution	2.2	1.6	.01	2.2	1.6	.01
Return of Decision Making to States and Localities	2.2	1.8	.01	2.3	1.7	.05
Ease of Application Process	1.5	1.3	.05	1.5	1.3	NS
New Federalism Philosophy as an Improvement over Categorical Grants	2.6	2.2	.01	2.7	2.4	NS

Source: Data compiled from authors' survey of senior federal officials conducted in 1976.

Note: In this table a *lower* mean score reflects a more *positive* attitude.

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[8] It might be argued that those selected in later years to these top positions would be more favorable to new federalism goals and objectives simply because they lacked the working knowledge of and commitment to previous policy alternatives. This might be the case with some political appointees; however, most officials promoted to top career positions have had years of experience with the federal government. We are primarily interested in this study in assessing the attitudes and characteristics of those career executives.

Summary and Conclusions

This article has attempted to assess the impact which a president can have, even in a short period of time, on the senior personnel of the federal bureaucracy. Of course the Nixon presidency was in many ways unique: a Republican president committed to eliminating or consolidating many social service programs and reducing many areas of federal bureaucratic discretion facing a bureaucracy accustomed to the considerable growth and social service orientation experienced during the Great Society days. Still, the Nixon presidency can be seen as representing a more general problem common to all presidents: the difficulty of gaining some degree of management control over the federal bureaucracy. The very fact that Nixon's struggles were more intense and more calculated than those of most presidents makes his a most fascinating and instructive case study. If the Nixon strategies were successful, future presidents might consider adopting similar tactics, however alien these may be to the concept of an independent career service. If the Nixon strategies failed, one might question whether any president could ever achieve control over the bureaucracy.

We may first conclude that the strategy of the Nixon White House was beginning to have a measurable effect. Those selected to the top bureaucratic positions during the Nixon years were more likely to be Republican and to favor Nixon's own goals and objectives than those selected earlier. Of course, this would be expected of political appointees, but we found this also to be true of career executives. These differences were not absolutes (many Democrats were promoted to the senior levels during the Nixon years and some were appointed by Nixon to the top positions), yet the data leave no doubt that Republicans and those supporting Nixon's policies were favored during the Nixon years. This is true of appointments to social service as well as all other agencies and departments.

Secondly, while marginal changes were found, the strategy pursued by the Nixon White House was doomed to insignificance from the outset. So few top officials are selected during any single presidency compared with the total number of senior level executives that any numerical impact which those selected can have must be slight. Of course, the White House hoped to overcome this problem by "encouraging" resignations, retirements, and transfers. Also, it might be argued that those few career officials selected by the White House were in "key" positions. Despite these tactics, even if every career executive selected during the Nixon years had been a Republican and totally dedicated to Nixon's programs and policies, they would have comprised only about 15 percent of all career officials. If future bureaucratic development moves toward further consolidation, waste-reduction, and personnel stability, a president cannot overlook the fact that far fewer positions will become vacant than will remain filled.

Thirdly, the data show that bureaucrats not strongly identifying with one partisan philosophy or the other nonetheless favor the president's program. This is especially interesting when compared with the Aberbach and Rockman data collected at the very beginning of the Nixon years which showed Independents to disproportionately resemble Democrats (and, combined, to reject the philosophies of a Republican administration). By the conclusion of the Republican era, we find Independents to resemble Republicans disproportionately (and, combined, to largely accept Nixon-era specific programs and philosophies). This is significant to presidential control because of the large numbers of bureaucrats calling themselves Independents. Aberbach and Rockman found 36 percent of the career officials in their sample to indicate an Independent status; we found 44 percent in our sample so responding. Using either figure, the number of Independents is far greater than the total number of career executives selected during any particular presidency.

We cannot conclude with certainty why Independents appeared more supportive of Nixon's programs in 1976 than in 1970. Certainly we do not believe that large numbers of senior career executives, many with years of experience and devotion to government service and many of whom had served under several presidencies, would suddenly and capriciously change their partisan leanings. We would not even argue that wholesale changes occurred in basic philosophic attitudes toward the general provision of social services (as measured by Aberbach and Rockman). Yet, the proportion of Independents who supported Nixon's *specific* programs and goals in 1976 clearly is higher than one would expect from the 1970 data.

Some of this accommodation is accounted for by White House influence over the selection process which, as we have documented, during the Nixon years placed those Independents in top career positions who were more likely to support the Republican party than were those selected before 1968. Some bureaucratic accommodation may reflect the growing support reached by 1976 for Nixon's domestic policy in the Congress and among state and local officials.[9] Perhaps these accommodations were also to some extent "self-motivated"–Independent executives may be more volatile than strong partisans and more likely to accept as their own the goals of the current administration. Whatever the answer (no doubt some combination of the above), by 1976 Independent career executives closely resembled Republicans in their support of the president's program. It is very likely that the large body of Independent bureaucrats may represent a considerable reservoir of potential presidential support.

The issues raised here have important implications in the broader context of national policy making and relate, as well, to longstanding debates about the appropriate role of the bureaucracy in a democratic society. Rational and consistent policy making and execution, it sometimes is argued, are best achieved by greater hierarchial control. Further, if such control over the federal bureaucracy can be enhanced by the selection of senior officials sympathetic to the president's policies, then it might seem desirable to extend the president's influence to senior career executives as well. From this perspective, the actions of the Nixon presidency may appear to represent simply a logical and even desirable extension of practices carried out by previous presidents. A Republican president facing both a Democratic Congress and a bureaucracy perceived to be unsympathetic to the president's policy directives might appear to be totally stymied without such influence. Aberbach and Rockman (1976, p. 467) have cautioned that the misdeeds of the Nixon administration should not be allowed to obscure the very real political problems the president faced, and Richard Nathan (1975, pp. 85–95) seems to suggest that were it not for Watergate, Nixon's bureaucratic tactics would have been judged successful and perhaps even desirable.[10]

Of course, the counter-argument–that the bureaucracy best serves democratic objectives by retaining its independence even in the face of strong presidential pressures–remains persuasive as well. From this perspective, policy making is more seriously threatened when control from the top inhibits the free exchange of ideas at the lower levels. The bureaucracy may be the best (perhaps the only) source of representation for some interests ignored by the elected branches of the government (one need only consider the attachment of social service organizations to HEW during the Nixon years). Indeed, Rourke (1969, p. 153) argues that bureaucratic politics has replaced party politics as the dominant decision-making arena in modern societies; unless the bureaucracy is free to generate innovative policy alternatives–occasionally even in conflict with elected representatives–the goals of democracy are ill served. According to Rourke (1969, p. 150), "A decision-making system subject to [rigid hierarchical control] is incapable of meeting all the requirements of a democratic society committed–in theory at least–to full and frank exploration of all options as a prerequisite to rational decisions."

Our data cannot resolve these debates over the "proper" role of the bureaucracy in a modern society. However, we believe the data presented here do demonstrate the near-impossibility of achieving presidential control through manipulation of the personnel system. In the span of one presidency, the total number of vacant positions at senior levels simply is too small for a president to have more than a marginal impact. The conclusion that absolute presidential control is unlikely should be particularly satisfying to advocates of a free and independent bureaucracy.

On the other hand, we find the "pull" of the presidency to be strong, indeed. Like Aberbach and Rockman, we find Democratic bureaucratic identifiers consistently less favorable to a Republican president than Republican identifiers. Yet, by 1976 we find substantial proportions of *all* party identifiers (Democrats, Republicans, Independents) sympathetic to Nixon's proposals. In fact, Independents as a group were found to be almost indistinguishable from Republicans in their support of Nixon's policies and programs. Independents and party identifiers combined assure either a Republican or a Democratic president substantial support at the senior career levels of the federal bureaucracy. The important point is that, if these trends are reflective of typical bureaucratic accommodation to a president's specific programs and goals, presidents can achieve a measure of bureaucratic support in spite of the relatively few vacancies to be filled during any given administration.

Thus, Aberbach and Rockman's perceptive observation is correct: "The framers of the Constitution, having given us little guidance [in the area of bureaucratic control and responsibility], have left these problems as part of their enduring legacy" (1976, p. 468). Yet, lacking constitutional directives we find that political realities provide considerable relief. The influence of the president is so strong as to sway even protected career service personnel. While maintaining its formal independence (and perhaps even basic philosophic differences), the bureaucracy still responds to specific presidential initiatives. To this extent a certain degree of presidential control is accomplished even in the absence of clear constitutional direction.

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[9] We are grateful to an anonymous reader for this insight.

[10] Nathan, actually, equivocates on this point, judiciously pointing out both sides of the issue. However, a fair interpretation of Nathan's arguments seems to imply sympathy for the goals, if not always the tactics, of the administrative presidency.

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Competition, Constituencies, and Welfare Policies in American States*

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This article examines the logic underlying standard formulations of the interparty competition hypothesis in the comparative state policy literature, suggests a reformulation which provides some new insights into the conditions under which we might expect state policies to change as a result of party characteristics, and undertakes an initial test of the reformulation.

I develop two propositions. The first is that party systems which divide the electorate along economic class lines will generate more generous welfare policies than party systems which do not so organize the electorate. The second is that within states with class-based electoral systems, change in welfare policy will be positively related to the degree to which the party or faction with lower- and working-class support gains control of government.

I analyze welfare policies of selected American states for the period 1938 to 1970. My analysis suggests that (1) the class basis of electoral politics does influence state welfare policies and (2) parties and factions which differ in their constituency bases produce different types of policies when they are in control of government.

For almost 20 years, political scientists have attempted to analyze systematically relationships among the policy outputs, political patterns, and socioeconomic characteristics of the American states. The focal point for much of this research has been the proposition that states with more competitive two-party systems will be more generous in their social welfare policies than will less competitive two-party states or one-party states.[1]

This proposition that the "have-nots" fare better under conditions of competitive party politics had its genesis in V. O. Key's *Southern Politics in State and Nation* (1949). Surveying the generally abysmal level of educational and social welfare programs in the South, Key concluded that the deprived condition of the poor was a product of disorganized, personalistic electoral politics. Republican opposition was minimal or nonexistent. Although Democratic primaries were frequently contested, elections were generally personalistic in nature and competing Democratic factions were typically based on personal followings and ad hoc coalitions which shifted from election to election. Competing factions lacked organization, cohesiveness, and continuity. Race relations provided the only issues; and these were issues on which there was general agreement among all who were allowed to participate in politics.

Key maintained that several consequences flowed from the disorganized, noncompetitive nature of southern politics. Voters were frequently faced with the task of selecting from among a confusing array of candidates who lacked enduring ties to particular policies or programs. Factions lacked a record of past performance on which voters could base their judgments. Campaigns lacked issues. Victors found it difficult to effect change and had little incentive to attempt to do so. According to Key (1949, p. 307),

> over the long run the have-nots lose in a disorganized politics. They have no mechanism through which to act and their wishes find expression in fitful rebellions led by transient demagogues who gain their confidence but often have neither the technical competence nor the necessary stable base of political power to effectuate a program.

Beginning with Lockard (1959), numerous scholars have tested Key's observation that the poor benefit from organized, competitive electoral politics by relating the closeness of electoral competition in American states to a variety of policy outputs. Dawson and Robinson (1963), Dye (1966), Fry and Winters (1970), Winters (1976), and others have suggested that interparty competition has little

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[1] Much of the research is reviewed in essays by Fenton and Chamberlayne (1969), Hofferbert (1972), and Lipsky (1968).

independent impact on policy outputs. Despite occasional reports to the contrary (e.g., Cnudde and McCrone, 1969), most studies have concluded that the effects of interparty competition are largely spurious and that a state's socioeconomic environment determines both its politics and its policies. I will examine the logic underlying standard formulations of the interparty competition (IPC) hypothesis, suggest a reformulation which provides some new insights into the conditions under which we might expect state policies to change, and undertake an initial test of the reformulation.

Party Competition and Welfare Policy

The general interpretation of the interparty competition hypothesis is that close interparty competition provides strong incentives for both parties to formulate policies for the benefit of the "have-nots." Virginia Gray (1976, p. 239) has summarized clearly the logic which apparently underlies most applications of the IPC hypothesis:

> "Have-nots" constitute the largest pool of potential voters. Party leaders, recognizing competition as a threat to the attainment or retention of office, will adopt a policy strategy directed toward the "have-nots." Hence, two-party competitive states will offer more policies beneficial to "have-nots" than will one-party noncompetitive states.

Thus, in a manner strongly reminiscent of Downs' *Economic Theory of Democracy* (1957), the standard interpretation conceives of voters as being distributed along a continuum of welfare policy preferences. The distribution of preferences is unimodal, but it is skewed toward more generous welfare policies because the disadvantaged make up such a large proportion of the population. In a competitive situation, both parties are encouraged to move their welfare policy formulations toward the midpoint of the distribution (which represents more generous welfare policies), because there they can acquire the most votes. In noncompetitive situations, however, parties do not necessarily move in that direction, because they do not need to fear retribution at the polls.

The assumption that both parties will appeal to the disadvantaged in a situation of close interparty competition is not necessarily valid. Another possibility is that one party will make its appeal principally to the disadvantaged while the other will seek support from established interests in society. There is certainly evidence that this has happened in some American states. According to John Fenton (1966), parties have divided the electorate along socioeconomic lines in the competitive states of the upper Midwest. In each of the states, the votes of the disadvantaged accrued disproportionately to the Democratic party, with the votes of more advantaged citizens going to the Republican candidates. In such a situation, there may be little reason for the Republicans to reward the "have-nots." If this is true, it follows that competition is important for social welfare policy only because it indicates that the party of the "have-nots" has the opportunity to gain control of the government and to formulate policies beneficial to its supporters. Social welfare policies will be liberalized only if that party gains sufficient power to implement programs. This being true, competition alone will not lead to more liberal welfare policies. If the non-lower-class party gains power, welfare programs may suffer. If control of state government is divided, the lower-class party may be unable to implement its policy preferences. In fact, if the lower-class party is dominant, low competition could lead to higher welfare benefits.

This would be possible under several sets of conditions. Under the first set, the population is distributed bimodally with respect to its policy preferences, some favoring liberal welfare policies, others favoring conservative ones. The disadvantaged presumably have liberal welfare policy preferences and constitute a significant proportion of the population favoring such policies. According to Downs, competitive parties would promote policies corresponding to the mid-points of the respective groupings. Movement toward the center would pose too great a threat of lost votes at the extremes. Although it is normally assumed in American politics that policy preferences are unimodal, a bimodal distribution might be expected among those voters whose preferences are most intense and to whom politicians are most likely to respond.

Another possibility is that parties do not base their policy strategies on the basis of perceptions of the policy preferences of the general public. Instead, party leaders view the public as composed of groups of supporters who provide varying degrees of support for the party. Each group is marked by some distinguishing characteristic; each group has its own needs and preferences; each attaches a different degree of importance to different areas of public policy; and each tends disproportionately to support one or the other party.

Party leaders know which groups have provided them with the greatest support in the past. They are attentive to their supporters' needs and preferences because they believe that (1) past performance is the best guide to future behavior in electoral contests and (2) the continuing loyalty of past supporters is crucial in electoral contests. Furthermore, the actions which reward one group may not be the actions which would antagonize other groups.

Class-based politics can create such a situation. The disadvantaged come to provide disproportionate support for party A; other voters provide disproportionate support for party B. The disadvantaged place heavy importance on the adoption of more generous welfare policies and party A responds with appropriate policies when it is in power. The middle- and upper-class supporters of party B either favor less generous welfare policies, or they prefer that the party devote its limited attention and effort to other policy arenas. Party B responds to its constituency with either lower welfare benefits or little change in welfare benefits.

Even with a unimodal distribution of voters' preferences and party leader attention to those preferences, it is quite possible that competing parties will develop fairly distinct policy orientations. If voters with more extreme policy preferences provide a disproportionate amount of financial support and campaign workers, if parties are dependent upon those resources in order to win elections, and if they believe that that support will not be forthcoming if they move too close to the middle of the preference distribution, then we could expect the parties to maintain somewhat distinct policy positions. In addition, if party activists interact primarily with those elements of the electorate which provide their greatest support, they will have biased perceptions of the intensity and direction of the electorate's policy preferences which will shape any rational calculations designed to maximize voter support for the party.

The argument that party competition may not be sufficient to promote more liberal welfare policies is fully consistent with V. O. Key's original formulation. Key was concerned not only with competition, but also with organization. He argued that competitive parties help organize political preferences and implement policy choices once a party is elected to office (Key, 1949, p. 308). Key stressed, in particular, the importance of political organization as a mechanism for the promotion of public policies favorable to the interests of the disadvantaged in society (1949, p. 307).

It follows that both parties need not compete for the votes of the lower classes in order for social welfare programs to become more generous. What is required is a mechanism for the expression of lower-class interests. One party can fulfill this function; if it achieves power, we would expect social welfare benefits to increase. If lower-class support is concentrated in one party, however, welfare benefits can be expected to increase only if that party gains sufficient power to implement programs it favors.

At least two studies can be interpreted to support this analysis of the conditions under which characteristics of party systems might be expected to affect welfare policies. In his study of *Midwest Politics* (1966), John Fenton found that the lines of electoral cleavage varied among six midwestern states, although each was characterized by vigorous two-party politics. In Minnesota, Wisconsin, and Michigan, party electoral support followed class lines, pitting the disadvantaged and their allies against those better off. In Indiana, Ohio, and Illinois, on the other hand, party cleavages were based on patterns of immigration and loyalties dating back to Civil War allegiances. Fenton found that the states with political conflict organized along lines of economic class produced more generous welfare policies than those in which political conflict was not so organized.

In a study of Louisiana politics (Jennings, 1977), I analyzed changes in general expenditures and welfare spending in Louisiana from 1928 to 1960. During that period, two organized factions of the Democratic party, Long and Reform, competed for power, dividing the electorate along socioeconomic lines. The Longs received their greatest support from poor farmers and the urban working class. The Reformers were supported principally by the urban middle and upper classes. Analysis indicated that Long administrations consistently produced more generous welfare expenditures (as well as much larger increases in general expenditures), even when controls were introduced for such variables as economic growth and regional patterns of change. There was little indication that fierce competition for office led Reformers to promote more generous welfare policies.

State Welfare Policy and the Lower-Class Electorate

The preceding analysis suggests a theory of state welfare policy which emphasizes the way in which parties and factions organize the electorate. It can be stated as follows:

The generosity of a state's social welfare policies depends upon the organization and mobilization of the lower classes in the electoral process. When a party or faction which receives substantial electoral support from the lower-class voters attains office, social welfare benefits should increase. When a party or faction which receives minimal support from the disadvantaged attains office, social welfare benefits should remain constant or decline. Party systems which organize the electorate along lines of class cleavage should generate more generous welfare policies than party systems which do not so organize the electorate, so long as the lower- or working-class party has enjoyed success in its pursuit of office. When a party system fails to organize the electorate along class lines, welfare policy is likely to have low salience and neither party will use welfare benefits to reward its supporters.

My basic argument is that the electoral coalition which supports a party determines that party's policy actions—at least with respect to social welfare policy. This means, in turn, that the composition of a lower-class coalition may affect the kinds of welfare policies stressed by the lower-class party. A lower-class party dominated by a rural component may reasonably be expected to emphasize different social welfare policies than a party drawing its strength from an urban, industrial working class. The former might stress old-age assistance; the latter might emphasize unemployment insurance and labor legislation.

Certain variables would obviously intervene between the electoral success of a lower-class coalition and the implementation of more generous social welfare policies. For example, the separation of powers and bicameral legislatures in the American states stand as possible barriers to the fulfillment of lower-class wishes. The efforts of a governor elected by a lower-class party to institute more generous social welfare policies may be stymied by one or both houses of a legislature dominated by the opposition party. This factor will be taken into account in the analysis to follow.

Investigations of the IPC hypothesis have frequently viewed divided government as an indicator of competitive party politics and have included the extent to which control of government is divided in indicators of competition (e.g., Ranney, 1976). Much of the literature thus assumes that divided control of government will stimulate higher welfare benefits. The assumption here, to the contrary, is that divided government frequently stands as a barrier to more generous welfare policies. Divided government will lead to more generous welfare policies only when it indicates that the representatives of the disadvantaged have gained a share of power. The most generous increases, however, should occur during periods of unified control by the disadvantaged.

Changes in Politics and Policy in Eight States

In order to test the validity of the hypothesized relationship between electoral organization and social welfare policy, I compare changes in politics and policy in eight selected states from 1938 to 1970. In particular, the following hypotheses are investigated.

PROPOSITION 1. *States with class-based electoral politics will score high on measures of welfare policy relative to states without class-based electoral politics.*

PROPOSITION 2. *In states with class-based electoral systems, change in welfare policy will be positively related to the degree to which the party or faction with lower- and working-class support gains control of government.*

Class-based and non-class-based political systems are distinguished by the fact that economic class provides the primary source of political conflict and voter division in the former, whereas other factors (e.g., region, patterns of migration into states) tend to override or subsume class differences in the latter. In its simplest form, class-based politics exists when the working class and the poor vote 100 percent for party A and the middle and upper classes vote 100 percent for party B. Obviously, the real world of politics is more complcated—some degree of class-based politics probably exists in all states. Thus, state systems can range from those with a total class basis for electoral conflict to those with no class basis for electoral conflict. In essence, however, class-based electoral conflict finds those who are low on the social scale providing disproportionate support for one party and those who are high on the social scale providing disproportionate support for the other party.

To operationalize this concept for the analysis of state politics, I would have found it useful to have had systematic evidence about how voters of different social class backgrounds cast their ballots in state election contests; however, such data are not available. Consequently, I relied upon descriptions and data in studies of the various states to categorize the party systems.[2] Included in the data used to categorize the party systems were (1) descriptions of political cleavages by those who have studied the states, (2) polling and opinion data indicating the preferences of different social groups, and (3) information about the social class background of party officials and legislators.

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[2] A review of the existing literature on these states and the evidence used to classify their parties and party systems is available upon request.

States were selected for analysis on the basis of published studies containing information about their party systems. The key criterion was that there be sufficient information available from which to make a reasonable determination about the nature of electoral divisions. This information could be either qualitative or quantitative, although the latter was preferable, and some of each was available for each state included in the analysis. Because I was primarily interested in studying changes over time and differences within each state between parties or factions relying on different bases of support, I weighted the selection of states to include more class-based than non-class-based states. Six—Connecticut, Louisiana, Massachusetts, Michigan, Minnesota, and Wisconsin—were selected because available accounts suggest that political parties and factions organized the electorate along class lines during the period under analysis. Two—Indiana and Virginia—were selected because of the absence of class-based electoral politics.

The selection process decreased the possibilities for making comparisons between class-based and non-class-based electoral systems, but it increased the number of comparisons that could be made between parties or factions within class-based systems. In addition, changes in the party systems of two states increase the number of comparisons which can be made between types of party systems. The early shift to class-based electoral cleavages in several of the states and discontinuities in the policy data precluded more analysis of this type. If we think in terms of party systems as the units of analysis, there are seven class-based systems and three non-class-based systems. The exact number of states selected for analysis was set somewhat arbitrarily at eight, but the decision to limit the number of states was based on the fact that time and resources constrained the number which could be included in the analysis.

The political characteristics of the selected states between 1938 and 1970 differ in interesting ways. Louisiana was a one-party state in which relatively stable and well-organized factions effectively organized the electorate along class lines and alternated in power from 1928 to 1960, when the class-based electoral system collapsed. Virginia was a one-party state in which one organized faction dominated the electoral system and effectively excluded much of the lower class from participation. There is some evidence that electoral politics began to divide the electorate along economic lines in 1965 in Virginia. Massachusetts and Connecticut apparently had party systems based to some extent on class lines. In these states, control of the governor's office shifted from party to party and control of state government was frequently divided. Close interparty competition characterized Indiana politics, but the electorate did not divide along lines of class cleavage. The evidence indicates that party competition divided the electorate along class lines in Michigan beginning in the mid-thirties, but that the Democratic party was unstable and poorly organized before the active involvement of the United Auto Workers in party affairs in the late 1940s. The existence of the Democratic-Farmer-Labor (DFL) party and its general lack of success until the 1950s makes Minnesota an interesting case. The history of Progressive politics and the eventual shift of Progressive support to the Democratic party characterize the class-based politics of Wisconsin.

Table 1 classifies the electoral systems of the states under study. Table 2 lists the factions and parties which have represented lower-class/working-class constituencies. While considering these classifications, one should bear in mind that first, it is not possible, given the available evidence, to distinguish degrees of difference among the class-based electoral systems, although one would assume that the importance of socioeconomic cleavages in electoral politics varies among states identified as having class-based systems. Second, it is not possible to distinguish the degree and importance of lower-class and working-class support for those parties and factions which have relied on such support. Third, the classification system ignores other cleavages and factors which have been important in the electoral politics of these states, such as ethnicity and size of place, because it is not possible to determine the extent to which they have affected the economic class basis of electoral politics.

Fourth, although this is not listed separately in the classifying scheme, available evidence indicates that farmers working marginal land in Louisiana and Minnesota considered themselves to be part of the disadvantaged population and contributed to the support of lower-class factions and parties. This was also true of the farmers in northern and northwestern Wisconsin. Elsewhere, farmers apparently considered themselves to be of higher status and contributed support to the parties and factions opposing working-class/lower-class coalitions.

The welfare policy indicators included in the analysis are per capita state welfare expenditures, welfare effort (per capita welfare expenditures divided by per capita income), recipient rate (total population divided by number of welfare recipients), expenditure per recipient, per recipient effort (expenditure per recipient divided by per capita income), and average unemployment insurance payment per recipient.[3]

Per capita state welfare expenditures are probably the single best indicator of welfare policy. They are, in effect, an index measure reflecting both the level of expenditures per recipient and the proportion of the population deemed eligible to receive such benefits. Thus, they are a theoretically relevant measure. They also show a high degree of intercorrelation with the other measures.

Table 1. Classification of Electoral Systems of Eight States, 1932–1970

State	Years During Which Electoral System Was: Class-Based	Years During Which Electoral System Was: Non-Class-Based
Connecticut	1932–1970	
Indiana		1932–1970
Louisiana	1932–1959	1960–1970
Massachusetts	1932–1970	
Michigan	1932–1970	
Minnesota	1932–1970	
Virginia	1965–1969	1932–1964
Wisconsin	1932–1970	

Source: Edward T. Jennings, Jr. (1977). "Class-Based Electoral Competition, Control of Government, and Welfare Policy Change in Selected American States," Ph.D. dissertation, Washington University.

Welfare effort and per recipient effort reflect the extent to which a state is willing to devote its resources to welfare benefits. This has often been considered an important dimension of policy in the comparative state politics literature. It also provides a quick and ready means of adjusting per capita figures for differences in economic resources.

In order to focus on state policy outputs, I have subtracted federal and local contributions in computing state welfare expenditures. I have used per recipient expenditure of state funds, instead of payment levels in the categorical assistance programs, because it was not possible to put together a continuous series of categorical payments minus federal contributions. Although federal policies surely provide a stimulus for state action, I am convinced that an analysis of *state* policy change has to focus on welfare spending and payments from *state* resources. Otherwise, we are analyzing the combined policies of federal, state, and local governments.

All spending figures are expressed in real dollars, using 1947–1949 as a base. This is important in terms of comparing state policies over time because we are interested in the delivery of a combination of welfare goods and services.

The welfare policy measures were collected and computed on a yearly basis, but my analysis will focus on the average values of the policy measures during each administration in each state. We are interested in changes which occur from administration to administration. The average values of the policy indicators during each administration provide useful summary measures of each state's welfare policies during each, and permit ready comparisons among administrations. Yearly figures for 1940,

Table 2. Factions and Parties Relying on Disproportionate Support of the Disadvantaged, by State

State	Faction or Party	Years
Connecticut	Democratic party	1932–1970
Louisiana	Long faction	1932–1956
Massachusetts	Democratic party	1932–1970
Michigan	Democratic party	1936–1970
Minnesota	Farmer-Labor party	1932–1944
	Democratic-Farmer-Labor party	1944–1970
Virginia	Godwin Democrats	1965–1969
Wisconsin	Progressive party	1932–1944
	Democratic party	1944–1970

Source: Edward T. Jennings, Jr. (1977). "Class-Based Electoral Competition, Control of Government, and Welfare Policy Change in Selected American States." Ph.D. Dissertation, Washington University.

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[3] Data sources included the appropriate yearly volumes of the *Statistical Abstract of the United States,* the *Social Security Bulletin,* and an annual Census Bureau publication which has been variously titled *Financial Statistics of the States, State Finances,* and *Compendium of State Government Finances.*

1950, 1960, and 1970 will be used to make comparisons among the states.

System Differences and Policy Differences

According to Proposition 1, states whose electoral systems reflect class cleavages should score relatively high on welfare policy measures compared to states whose electoral systems do not reflect such cleavages. Thus, Louisiana, Connecticut, Massachusetts, Michigan, Wisconsin, and Minnesota should score relatively high on welfare policy measures; Virginia and Indiana should score relatively low. An analysis of each state's score on each policy measure for the years 1940, 1950, 1960, and 1970 indicates that the six states with class-based electoral systems score consistently higher than the two states without such systems. On these six measures, Virginia ranks seventh or eighth in 21 out of 24 cases. For two of the cases in which it does not rank seventh or eighth, there are reasons to suspect that the welfare spending data are reported inaccurately in the *Financial Statistics of the States* (1941).[4] On the six measures during the four years reported here, Indiana ranks seventh or eighth in 19 out of 24 cases.

Not only do the two states without class-based electoral politics generally occupy the lowest-ranking positions among these states on measures of welfare policy, but their scores are also generally far below those of the other states. For example, Wisconsin ranks sixth among the eight states on per capita state welfare spending in 1950, but its expenditures are 78 percent higher than those of Indiana. In 1960, sixth-ranking Minnesota has per capita welfare expenditures 164 percent higher than those of seventh-ranking Indiana. In 1950, Minnesota is spending almost twice as much per recipient of welfare benefits as is seventh-ranking Indiana. Differences of such magnitude are common for real per capita state welfare expenditures and per recipient expenditures. In terms of welfare effort, the sixth-ranking state scores nearly five percent higher than Indiana or Virginia in 1940, 104 percent higher in 1950, 146 percent higher in 1960, and 36 percent higher in 1970. In terms of per recipient effort, Virginia and Indiana generally score 30 to 40 percent less than the other states. These data provide initial support for Proposition 1.

Overall Ranking of Eight States. As was noted earlier, per capita welfare expenditures is probably the best single indicator of welfare policy. The average ranking of the states on per capita state welfare expenditures for these four years conforms perfectly with what we would expect, given the characteristics of their political systems. Table 3 presents the overall rankings of the states on per capita welfare expenditures for the years 1940, 1950, 1960, and 1970.

Massachusetts, Connecticut, and Louisiana, which occupy the first three positions, were characterized by class-based electoral politics. They are the states in which the party or faction with lower-class support enjoyed the greatest success. In Massachusetts and Connecticut, Democrats frequently inhabited the governor's mansion, controlled at least one branch of the state legislature for extended periods, and occasionally enjoyed unified control of government. In Louisiana, governors exercised extensive power and normally exercised considerable control over legislative decision making during the first two years of their terms.

Michigan was characterized by class-based electoral conflict. Although the Democratic party did not achieve unified control of state government, it had more frequent access to decision-making power either through its control of the governor's office or the lower house of the legislature than did the Democratic party in Wisconsin or the DFL in Minnesota. Thus, we would expect it to fall in between Louisiana, Massachusetts, and Connecticut, on the one hand, and Minnesota and Wisconsin, on the other.

Minnesota and Wisconsin also were characterized by class-based electoral politics during this period; consequently, we would expect them to have more generous welfare policies than Indiana and Virginia, which were characterized by non-class-based politics. For reasons just suggested, we would expect Minnesota and Wisconsin to have welfare policies somewhat less generous than the other states with class-based electoral systems.

Changes in Systems and Changes in Policy. Another approach by which we can assess the

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[4] These two cases occur on welfare output measures for 1940. According to data reported in *Financial Statistics of the States* (various years), Virginia welfare expenditures were: $1.7 million in 1939; $6.2 million in 1940; and $4.3 million in 1941. This wide fluctuation does not appear to be likely. In addition, federal contributions to public assistance programs in Virginia do not fluctuate in a corresponding manner: $0.7 million in 1939; $1.4 million in 1940; and $2.0 million in 1941.

Table 3. Overall Ranking of Eight States on Real Per Capita State Welfare Expenditures for the Years 1940, 1950, 1960, and 1970

State	Rank
Massachusetts	1
Louisiana	2
Connecticut	2*
Michigan	4
Minnesota	5*
Wisconsin	5*
Indiana	7
Virginia	8

Source: Data compiled from appropriate yearly volumes of U.S. Department of Commerce, Bureau of the Census, *Statistical Abstract of the United States*, Washington, D.C.: Government Printing Office; U.S. Department of Health, Education and Welfare, Social Security Administration, *Social Security Bulletin*, Washington, D.C.: Government Printing Office; and an annual publication of the Census Bureau which has been variously titled *Financial Statistics of the States, State Finances*, and *Compendium of State Government Finances*, Washington, D.C.: Government Printing Office.

*Tie ranking.

validity of Proposition 1 is to look at what happens to a state's welfare policies as its political system shifts from non-class-based to class-based electoral conflict. Unfortunately, the six states in this study which have been characterized by class-based electoral conflict cannot be studied in this way because they developed class-based electoral politics prior to the time for which a consistent series of welfare policy measures is available. In most of them, available evidence indicates that the electorate began to divide along lines of class cleavage in the late 1920s or early 1930s. We lack a consistent series of before and after data on which to base an analysis of the impact of such change in electoral politics.

Studies by Howard (1971) and Havard (1963) suggest, however, that Louisiana shifted away from class-based electoral politics in the 1960 election, so we may examine what happened to its welfare policies after that date. As can be seen in Table 1, most measures of welfare policy in Louisiana declined during the decade. Between 1960 and 1970, real per capita state welfare spending fell from $13.80 to $11.30, welfare effort fell from .1086 to .0614, expenditures per recipient fell from $178.42 to $117.32, and recipient effort fell from .140 to .064. Only the recipient rate and unemployment insurance payments increased. The recipient rate increased during the last three years of the decade, when such rates were increasing rapidly in many states. As we shall see later, unemployment insurance benefits seem to increase in all states in response to increases in wealth, regardless of political characteristics. So the Louisiana data, on the whole, provide fairly strong support for Proposition 1.

Virginia provides another testing ground. Although its politics generally either ignored or suppressed class cleavages, the last governor of the 1960s came to office with the support of organized labor and blacks. Thus Virginia's electoral politics was beginning to follow lines of economic and social cleavage. Did it make a difference? Yes, according to the data. The last budget prepared under a Byrd organization candidate was for the 1966 fiscal year. The ensuing Godwin administration prepared the fiscal year budgets for 1967–1970. During that administration, welfare policy outputs increased substantially: per capita state welfare spending by 151 percent; welfare effort by 117 percent; the recipient rate by 50 percent; per recipient expenditures by 68 percent; and per recipient effort by 45 percent. Unemployment insurance payments showed a modest 12 percent increase. In each case, these increases are greater than changes in national average values of these policy indicators during this period.

Based on the cross-sectional and longitudinal analyses of this small, nonrandom sample of state political systems, there appears to be some evidence to support the hypothesis (Proposition 1) that electoral systems organized along class lines will produce more generous welfare policies than those which are not so organized. These findings hold even for the measure termed *welfare effort*. This measure adjusts for differences in per capita income by expressing welfare expenditures as a proportion of per capita income. A summary ranking for the eight states on welfare effort for the years 1940, 1950, 1960, and 1970 places them in the following order: Louisiana, Massachusetts, Michigan, Connecticut, Minnesota, and Wisconsin. Virginia and Indiana tie for the last spot. Thus, differences in resources do not appear to account for differences on the other policy measures.

Electoral Support and Change in Policy

In this section, we will review evidence bearing on Proposition 2. Does it make a difference if the disadvantaged gain access to the councils of government through the mechanism of a political organization which they support? The analysis focuses on the six states which I have identified as characterized by class-based electoral cleavages.

Table 5 presents the mean change in welfare policy averages, by type of control of government, for each of the six states. Three types of control are possible: (1) unified control by the middle-class party or faction; (2) divided control; and (3) unified control by the party or faction relying on lower- and working-class support. In Louisiana it is not possible to study differences based on degree of control because we have no consistent information about factional divisions in the legislature. Available studies do suggest, however, that the governor was able to exert considerable influence, and we will compare changes under Long, Reform, and post-Long (1960s) administrations. There was no time during the period under study when the DFL in Minnesota or the Democratic party in Wisconsin or Michigan controlled both

Table 4. Louisiana Scores for Six Welfare Policy Measures, 1960–1970

Year	Per Capita Welfare Expenditures	Welfare Effort	Recipient Rate	Expenditure Per Recipient	Per Recipient Effort	Unemployment Insurance Benefit
1960	$13.80	.1065	.0773	$178.42	.140	$23.96
1970	$11.30	.0614	.0962	$117.32	.064	$26.40

Source: Data compiled from appropriate yearly volumes of U.S. Department of Commerce, Bureau of the Census, *Statistical Abstract of the United States,* Washington, D.C.: Government Printing Office; U.S. Department of Health, Education and Welfare, Social Security Administration, *Social Security Bulletin,* Washington, D.C.: Government Printing Office; and an annual publication of the Census Bureau which has been variously titled *Financial Statistics of the States, State Finances,* and *Compendium of State Government Finances,* Washington, D.C.: Government Printing Office.

Table 5. Mean Change in Policy Indicator Averages, by Type of Control, Six States, 1938–1970

Type of Control	Per Capita Welfare Expenditure	Welfare Effort	Recipient Rate	Expenditure Per Recipient	Per Recipient Effort	Unemployment Insurance Benefit
Louisiana						
Long	$6.07	.0527	.0267	$23.98	.014	$1.90
Reform	–1.21	–.0052	–.0028	–18.25	–.072	1.63
Post-Long	–1.05	–.0177	.0049	–21.74	–.027	1.62
Michigan						
Divided	.91	.0004	–.0001	28.38	.002	1.39
Republican	–.41	–.0081	.0010	–24.73	–.038	–.19
Minnesota						
Divided	.13	–.0013	.0000	5.40	–.005	1.10
Republican	–.54	–.0062	–.0020	6.23	–.008	.84
Wisconsin						
Divided	1.22	.0052	.0000	60.89	.026	1.64
Republican	.23	–.0013	–.0012	12.54	.000	.86
Connecticut						
Democratic	3.93	.0111	.0043	49.50	.004	3.80
Divided	.37	–.0029	.0002	11.39	–.016	2.35
Republican	.59	.0045	–.0001	58.34	.039	1.17
Massachusetts						
Democratic	2.68	.0096	.0003	86.59	.031	2.43
Divided	2.55	.0087	.0019	36.57	.010	.39
Republican	–.16	–.0052	–.0025	13.02	–.003	3.60

Source: Data compiled from appropriate yearly volumes of U.S. Department of Commerce, Bureau of the Census, *Statistical Abstract of the United States,* Washington, D.C.: Government Printing Office; U.S. Department of Health, Education and Welfare, Social Security Administration, *Social Security Bulletin,* Washington, D.C.: Government Printing Office; and an annual publication of the Census Bureau which has been variously titled *Financial Statistics of the States, State Finances,* and *Compendium of State Government Finances,* Washington, D.C.: Government Printing Office.

houses of the legislature and the governor's office.[5] Thus, they were characterized only by divided control or unified Republican control. Only in Massachusetts and Connecticut did the party with working-class support, the Democrats, manage to gain unified control of government during this 32-year period.

Several things stand out in the data reported in Table 5. The first is that welfare policy change is more generous on more measures when control of government is divided or when the working-class party has unified control than when the middle-class party has unified control. In Louisiana, change is more generous under the Long administrations than under the Reformers or the post-Long governors on all six policy measures. In Wisconsin, change is more generous on all six measures during divided control of government than during unified Republican control. In Minnesota, the generosity of welfare policy change is greater on five of six measures when the DFL controlled part of the government. In Michigan, the relationship is as expected for each measure except recipient rate.

In Connecticut, the relationship is as expected in 12 of 18 cases. Welfare policy change during periods of complete Democratic control is more generous on all six measures than during periods of divided control; it is also more generous than during periods of unified Republican control on four measures. However, change during periods of divided control is more generous than during periods of unified Republican control on only two measures.

In Massachusetts, the relationship is as expected in 15 of 18 cases. The deviations occur on recipient rate (which increases more rapidly during periods of divided control than during periods of unified Democratic control) and on unemployment insurance benefits (which experience the largest increase during periods of unified Republican control).

The data in this table provide fairly consistent support for the proposition that parties with lower- and working-class support will provide more generous welfare policies than parties without such support. The differences are not always large, and there are some deviant cases, but the expected finding occurs in more than 80 percent of the cases.

Economic Change and Party Differences. We can now address our attention to the question of whether or not changes in welfare policy during different types of regimes simply reflect changes in economic resources which occur during different regimes. This is an important question because comparative state policy analyses have found that (1) differences in economic resources frequently help account for differences in public policy and (2) correlations of policy variables with political variables are frequently reduced when controls for economic resources are introduced.

Because of the limited number of cases under investigation (a maximum of 15 in any one state), we cannot introduce statistical controls for a variety of different measures. We can, however, specify a simple set of policy models and estimate the parameters of those models through least squares regression. Differences among the model parameters and in the proportion of variance explained can be used to assess the independent impact of party or factional differences.

Two regression equations were estimated to assess the differences between types of political control when controlling for economic change.

$$\Delta P_{it} = a + b\,\Delta E_t, \text{ where} \tag{1}$$

ΔP_{it} = change in policy i from period $t-1$ to period t and ΔE_t = change in per capita personal income from period $t-1$ to period t.

$$\Delta P_{it} = a + b\,\Delta E_t + cD_t + dM_t, \text{ where} \tag{2}$$

ΔP_{it} and ΔE_t are as in the previous equation, and D_t = party or factional control of government. If unified working-class party control is present, D_t has a value of 1; otherwise, it has a value of 0. M_t = party or factional control of government. If control of government is divided between parties or factions, $M_t = 1$; otherwise, $M_t = 0$.

As tests of the proposition, we want to know two things. Does the amount of variance explained (R^2) increase as we move from Equation 1 to Equation 2? Do the coefficients of the dummy variables have positive values?

For purposes of convenience, we shall refer to Equation 1 as the *economic resources* model and Equation 2 as the *partisan control* model. The economic resources model asserts that changes in welfare policy are a product of changes in economic resources. Equation 2 asserts that changes in welfare policy are a product of changes in economic resources and the extent of control of government by parties or factions relying upon electoral support of "have-nots."

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[5] Although Minnesota had a nonpartisan legislature during the period under consideration, it was assumed, for purposes of analyzing the effects of partisan control of government, that the Liberal and Conservative caucuses in the legislature were surrogates for the political parties.

Variance Explained by Models. Table 6 presents the R^2 (amount of variance explained) for each model for each policy measure for each of the six states. There are a number of things to be said about these data.

First, changes in per capita personal income do not account generally for much of the variance in welfare policy change. This, of course, varies from policy measure to policy measure and from state to state. Change in income accounts for 20 percent or more of the change in policy in three or more states for only three policy measures—per recipient unemployment insurance benefits, welfare effort, and per recipient effort. Change in income has the greatest explanatory power for changes in unemployment insurance change in five of the six states. Moreover, economic resources explains best those policy indicators which are based on money. The two variables for which income provides the least explanatory power (per capita welfare expenditures and welfare rate) embody dimensions of policy other than those which are expressed purely in dollar terms. Finally, change in income accounts for 20 percent or more of the variance in more than three policy measures in only one state—Connecticut.

The data do provide support for the original hypothesis. Even when we control for changes in economic resources, partisan control of government apparently makes a difference in welfare policy change. In 14 of 36 cases, the amount of variance explained by the partisan control model is more than twice as large as the amount of variance explained by the economic resource model. In eight additional cases, the amount of variance explained increases by 10 percent or more.

The data also support the suggestion that the ability of lower- and working-class parties or factions to gain complete control of government is an important determinant of the extent to which it will achieve its goals. The addition of partisan control dummy variables to the economic resource model provides the greatest increase in explanatory power when applied to Massachusetts and Connecticut. These are the two states in which the Democratic party was able to gain unified control of government.

Regression Coefficients. Examination of the regression coefficients reported in Table 7 demonstrates that changes in per capita income are generally related in a negative fashion to changes in welfare policy. Increases in income apparently lead to less generous welfare policies. This is most apparent in the case of recipient rate, where b is negative for all states. It is not true, however, in the case of unemployment insurance where the sign of b is negative for only one state. The relationship between changes in income and changes in per recipient expenditures is less clear. In half of the cases, the sign of b is negative, in the other half, it is positive.

The regression coefficients themselves provide substantial additional support for the thesis. In 30 of 36 cases, d is positive. Thus, 83

Table 6. Amount of Variance in Welfare Policy Indicators Explained by Economic Resources and Partisan Control Models

	Dependent Variables: Amount of Variance Explained by Economic Resources Model (E) and Partisan Control Model (P)											
	Per Capita Welfare Expenditure		Recipient Rate		Expenditure Per Recipient		Welfare Effort		Recipient Effort		Unemployment Insurance Payment	
State	E	P	E	P	E	P	E	P	E	P	E	P
Connecticut	.07	.43	.04	.36	.27	.35	.36	.59	.75	.79	.37	.40
Louisiana	.33	.39	.14	.28	.70	.71	.09	.11	.59	.73	.02	.02
Massachusetts	.04	.15	.03	.18	.08	.15	.00	.14	.00	.08	.27	.42
Michigan	.07	.19	.05	.06	.05	.26	.36	.48	.23	.40	.31	.33
Minnesota	.00	.02	.12	.28	.04	.05	.11	.18	.04	.22	.22	.29
Wisconsin	.04	.17	.43	.46	.04	.22	.28	.41	.03	.03	.27	.29

Source: Data compiled from appropriate yearly volumes of U.S. Department of Commerce, Bureau of the Census, *Statistical Abstract of the United States,* Washington, D.C.: Government Printing Office; U.S. Department of Health, Education and Welfare, Social Security Administration, *Social Security Bulletin,* Washington, D.C.: Government Printing Office; and an annual publication of the Census Bureau which has been variously titled *Financial Statistics of the States, State Finances,* and *Compendium of State Government Finances,* Washington, D.C.: Government Printing Office.

Table 7. Regression Coefficients for Partisan Control Model

State	Intercept	*b*	*c*	*d*	R^2
Dependent variable– per capita state welfare spending					
Connecticut	.331	–.006	4.575	1.097	.43
Massachusetts	–1.343	.010	2.467	3.231	.15
Louisiana	3.029	–.023		1.986	.39
Michigan	–.058	–.005		1.498	.19
Minnesota	–.182	.00002		.307	.02
Wisconsin	.461	–.0027		1.060	.17
Dependent variable–welfare effort					
Connecticut	.2355	–.0048	1.690	.357	.59
Massachusetts	–.6051	.0007	1.452	1.431	.14
Louisiana	2.0650	–.0140		.943	.48
Michigan	–.2628	–.0069		1.107	.48
Minnesota	–.1810	–.0039		.538	.18
Wisconsin	.2720	–.0048		.779	.41
Dependent variable–change in recipient rate					
Connecticut	–.0015	–.0000089	.0074	.0034	.36
Massachusetts	–.0022	–.0000040	.0030	.0043	.18
Louisiana	.0078	–.0000580		.0139	.28
Michigan	.0016	–.0000060		–.0010	.18
Minnesota	–.0013	–.0000150		.0031	.28
Wisconsin	.0010	–.0000270		.0019	.46
Dependent variable– change in expenditure per recipient					
Connecticut	51.71	–.148	22.85	–13.38	.35
Massachusetts	–11.44	.208	65.65	34.29	.15
Louisiana	25.08	–.236		4.34	.71
Michigan	–15.94	–.111		57.27	.26
Minnesota	–.36	.077		–.411	.05
Wisconsin	7.10	.065		46.65	.22
Dependent variable– change in per recipient effort					
Connecticut	.0312	–.000200	.0037	–.0147	.79
Massachusetts	–.0037	.000004	.0341	.0132	.08
Louisiana	.0049	–.000400		.0429	.73
Michigan	–.2628	–.006890		1.1073	.48
Minnesota	–.0007	–.000040		.0011	.03
Wisconsin	.0048	–.000060		.0275	.22
Dependent variable– change in unemployment insurance payments					
Connecticut	1.909	.016	–.899	–2.184	.40
Massachusetts	2.136	.012	–.827	–2.743	.42
Louisiana	1.974	–.002		.023	.02
Michigan	–.553	.009		.847	.33
Minnesota	–.529	.010		.333	.22
Wisconsin	.190	.009		.505	.29

Source: Data compiled from appropriate yearly volumes of U.S. Department of Commerce, Bureau of the Census, *Statistical Abstract of the United States,* Washington, D.C.: Government Printing Office; U.S. Department of Health, Education and Welfare, Social Security Administration, *Social Security Bulletin,* Washington, D.C.: Government Printing Office; and an annual publication of the Census Bureau which has been variously titled *Financial Statistics of the States, State Finances,* and *Compendium of State Government Finances,* Washington, D.C.: Government Printing Office.

Note: b is coefficient for economic change; *c* is coefficient for total control by party with working-class support; *d* is coefficient for divided control of government.

percent of the time, divided control of government is associated with more generous welfare policies. In 10 of 12 cases, *c* is positive. This means that unified Democratic control is associated with more generous policy 83 percent of the time. In addition, in 8 of 10 cases (excluding unemployment insurance for which both coefficients are negative), the size of *c* is greater than that of *d*. These findings suggest that Democrats promote more generous welfare policies in these states; furthermore, party control of government appears to be a crucial determinant of party success in implementing policies favorable to supporters.

Controlling for External Influences. It is possible that the relationship between political control and policy change simply reflects an association between types of control and external events or forces which might lead to welfare policy change. For example, Republican governors may have been in office during periods when changes in unemployment reduced the need for welfare support programs. Democrats may have controlled government in these states during periods when proliferating federal grant programs freed additional state funds for welfare spending programs. Changes in the economy and federal policy changes can have an impact on state welfare policies, as can such factors as civil disturbances or a growing willingness on the part of potential recipients to claim benefits for which they are eligible. It could be argued, for example, that increased federal welfare spending would reduce the need and demand for state welfare expenditures. If Republicans were in office when this happened, it could be argued that changes in state welfare policy under Republican administrations are a response to these other changes, rather than a reflection of the party's constituency base of support.

One way to assess this possibility is to look at changes in the ratio of each state's policy scores to national average scores of state policy measures. If changes in the economy or federal policy are leading to a reduced demand or need for welfare support measures, the national average scores should reflect this fact. If state administrations are simply responding to such forces instead of reflecting their constituency bases of support, there should be no change in the state's policy scores relative to the national average. If the constituency base of support makes a difference in policy change despite forces that operate across all states, then

Table 8. Average Change in Ratio of Individual State Per Capita Welfare Expenditures to National Average State Per Capita Welfare Expenditures, Type of Control, Six States, 1938–1970

State	Type of Control	Average Change in Per Capita Welfare Expenditure Ratio
Louisiana	Long	.792
	Reform	.037
	Post-Long	–.566
Michigan	Divided	.046
	Republican	–.092
Minnesota	Divided	–.041
	Republican	–.035
Wisconsin	Divided	.087
	Republican	–.003
Connecticut	Democratic	.054
	Divided	.053
	Republican	.040
Massachusetts	Democratic	.136
	Divided	.105
	Republican	.071

Source: Data compiled from appropriate yearly volumes of U.S. Department of Commerce, Bureau of the Census, *Statistical Abstract of the United States*, Washington, D.C.: Government Printing Office; U.S. Department of Health, Education and Welfare, Social Security Administration, *Social Security Bulletin*, Washington, D.C.: Government Printing Office; and an annual publication of the Census Bureau which has been variously titled *Financial Statistics of the States, State Finances*, and *Compendium of State Government Finances*, Washington, D.C.: Government Printing Office.

changes in the state/nation ratio should vary by type of administration or control of government.

In investigating this question, we will focus on per capita state welfare expenditures, since that is the best single indicator of welfare policy. Table 8 presents average change in the state/nation ratio, by type of control.

The data in this table indicate that the relationship between the constituency base of support of parties and factions is reflected in welfare policy change, even when we control for the possible effects of political, social, economic, and policy changes on the national level. In every case except Minnesota, the relationship is as we would expect. More generous change relative to the national average occurs under Long governors in Louisiana and during periods of divided control in Michigan and Wisconsin. In Massachusetts and Connecticut, the most generous change relative to the national average occurs during unified Democratic control, the next most generous during periods of unified Republican control. Only in Minnesota is the thesis contradicted. Average change in Minnesota relative to the national average is negative under both unified Republican and divided control, but the decline in the state's relative position is greater under divided control than it is during unified Republican control.

Further Considerations

The weight of the evidence produced by this analysis provides strong support for the thesis that the nature of partisan alignments and partisan control of government under conditions of class-based politics influence welfare policy outcomes. Class-based and non-class-based political competition produce quite different sets of welfare policy outcomes in the eight states included in this analysis. Control of government appears to be important when parties or factions divide the electorate along lines of economic cleavage.

Although the propositions are supported by the data, a large proportion of the variance in the welfare policy measures is not explained by the statistical models. This reflects significant variations under each type of control. It probably suggests the need to include additional variables in a fully specified model of welfare policy change and the need to develop more sensitive and refined data on the composition of party constituencies. Some of the factors, both theoretical and operational, which may have influenced the findings are considered in the following paragraphs.

One reason that more of the variance was not explained may stem from the rather primitive nature of the political measures employed. Given the shortcomings of available data, it was necessary to use dichotomous classifications of party systems and the bases of party or factional support. This means that the political measures are insensitive to the degree to which electoral politics is organized along class lines and the degree to which a particular party or faction relies upon working- and lower-class support in any state or in any year—a shortcoming which makes it impossible to account for significant policy variations within each type of electoral system or regime.

A second possibility which has to be raised is that changes in welfare policy may well be shaped by the salience of welfare needs as a political issue and by elite ideologies, neither of which are taken into account in this study. Louisiana is the one state for which we have consistent information about campaign issues and the ideologies of political elites (Sindler, 1956; Howard, 1971). Welfare policy was an issue in every election in Louisiana during the period of class-based politics. The political rhetoric of the Longs and their supporters was oriented toward welfare-state liberalism; the rhetoric of the Reformers emphasized "good government" issues and fiscal responsibility.

Thus, in Louisiana, the class basis of electoral support, the saliency of welfare policy, and the political rhetoric of candidates reinforced each other. This was followed by clear and substantial differences in the pattern of welfare policy change under different types of administrations. Information on the reinforcing or non-reinforcing nature of these variables in other states might enhance the analysis.

Another possible factor to consider is the fact that political issues may seldom be cast in terms of class or group benefits in American state politics. This is related to the preceding concern with the salience of welfare policy issues and the nature of elite ideologies. If political issues are cast not in class or group terms, but are phrased to cut across class lines, then there would be less reason to expect policy change under different regimes to reflect differences in the class basis of electoral support.

It is also possible that the relative size of class groups might make a difference in welfare policy outputs. When the "have-not" group is large, the middle-class party may need to offer more liberal programs in order to win elections. When the "have-not" group is small, the party which relies on its support may have to modify its welfare policy demands.

Conclusions

To some extent the findings reported here support the standard hypothesis that interparty competition itself has policy consequences. The averages reported here obscure the fact that there are some periods of Democratic control or divided control when welfare generosity declines; further, there are periods of Republican control when welfare generosity increases. Even in terms of the averages, control by the party or faction relying on middle- and upper-class support is associated with increased generosity of welfare policy indicators in 38.8 percent of the cases. These findings suggest two things: (1) competitive, class-based politics can cause both parties or factions to move toward increased welfare benefits; (2) there may be factors in a situation of class-based politics which can pull the party or faction of the disadvantaged away from generous welfare policies.

The hypothesis which I put forth originally was based upon alternative conceptions of electoral competition, each predicated upon the existence of two organized, competing elites. One formulation assumes that class-based politics divides the electorate into opposing camps with sufficiently different policy preferences that politicians would not be attracted toward the middle on policy issues. An alternative formulation is that parties do not look at the entire population when formulating policies because they possess very limited information about the distribution of policy preferences among the public. Instead, they look at the composition of their electoral coalition and devise policies as payoffs to the components of those coalitions. In a situation of class-based politics, parties relying upon lower-class support would reward their followers with more generous welfare policies upon attaining office. Parties relying upon the votes of more advantaged citizens would reward their followers either with policy decisions unrelated to welfare programs or with less generous welfare policies, depending upon the salience of welfare policy to their followers.

Either of these formulations would support Proposition 2. How, then, do we account for the deviant cases? In addition to the possible factors previously mentioned, one possibility is that there is a group of voters who are not part of the primary electoral coalition of either party. These voters vary over time with respect to their preferences for welfare policy and they hold the balance of power between the competing parties. Since the parties must rely upon this group of voters in order to attain electoral success, parties must adapt their policies to the preferences of the swing voters, if they are to have the opportunity to achieve any of their goals.

When welfare policy is salient to the swing voters, parties must adapt their welfare policies to take this into account. If the preference of the group is for more generous policies, parties of the disadvantaged will have their existing policy tendencies reinforced. Parties of the advantaged will be forced to adopt more generous welfare policies, if they desire electoral success. If the preference of the swing voters is for less generous policies, parties of the disadvantaged will be forced in that direction, and parties of the advantaged will have their natural tendencies reinforced.

Under this formulation, changes in welfare policy would depend upon (1) the normal constituency of the party in power, (2) the saliency of welfare policy decisions to swing voters, and (3) the welfare policy preferences of swing voters. The formulation also assumes organized electoral competition which is sufficiently close that competing political elites can have reasonable opportunities to gain office.

Party Labels and Policy Analysis. This study represents an attempt to contribute to our understanding of the conditions under which interparty competition and party control make a difference in policy outcomes. Others have also explored the relationship between party control and policy outputs. In his wide-ranging study, Dye (1966) found little independent relationship between party control and policy outputs in the states. In a recent study, Winters (1976) concluded that partisan control makes little difference if redistribution is the focus of analysis. Fry and Winters (1970) reached a similar conclusion in an earlier study of redistribution.

Such findings are not surprising. Party labels have been used as policy relevant variables; however, the important thing is not the label, but the extent to which parties vary in policy-relevant attributes. In other words, if partisan control is to make a difference in policy outputs, it can only be because parties with different designations also differ in theoretically important ways. One such theoretically important difference would be the constituency base of party support. Recognizing the importance of this, Winters (1976, pp. 601–02) suggests that partisan control will affect the direction of budgetary redistribution because the parties rely on different groups of voters for support. Winters assumed, for purposes of analysis, that the Democratic and Republican bases of state party support would be relatively constant across the 34 northern states included in his analysis. Although the data are not available to conduct a thorough analysis of state party alignments, available studies do suggest that there are significant differences in partisan cleavages among these states. This being so, the party labels do not indicate comparable differences in party support from state to state, and relationships between partisan control and policy change are likely to be less than we might otherwise expect.

Party constituency is one policy-relevant attribute of party systems. Other relevant dimensions might be the political values and ideologies of the leaders of competing parties and the degree of party cohesion. Only if parties vary in these types of characteristics would we expect policy outputs to vary with differences in control of government. Party labels, as such, are theoretically meaningless. Once we identify the attributes of parties which might be important in policy development and develop information about the degree to which parties in different states are characterized by those attributes, we can begin to examine more carefully whether parties which vary in the extent to which they are characterized by the relevant attributes also vary systematically in their policy outputs.

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Scale Fitting in the Psychometric Model of Judicial Decision Making*

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The psychometric model, developed by Glendon Schubert, is a widely accepted way of conceptualizing judicial decision making. It entails the representation of justices' ideal points in a multidimensional space. A vector (representing the ordering of the justices derived from scalogram analysis) is projected through this space to aid in interpreting the underlying attitudinal dimensions of interagreement. Despite the apparent rigor of this model it contains certain indeterminacies, as no mathematical procedure has been used to determine the location of the scale vectors. Consequently ad hoc hand procedures which may yield suboptimal solutions have been developed.

This article provides solutions for the problem of fitting scale vectors to multidimensional configurations. These solutions are developed for the problem where the scale analogue is treated at the measurement level of interval or ordinal values. In order to demonstrate the advantages of using these procedures, we undertake a reanalysis of Schubert's results (for which he used ad hoc procedures). In one-half of the cases analyzed, the results reported by Schubert are suboptimal when evaluated in terms of the coefficient he sought to maximize. The mathematical solutions and the associated computer programs give greater rigor to the psychometric model and provide a response to the criticism that scale fitting in the psychometric model is subjective and inconsistent with the premises of the behavioral approach to judicial decision making.

Since Glendon Schubert initially proposed the psychometric model of judicial decision making in 1961, this model has been applied to the analysis of a large segment of the United States Supreme Court's decisions made during the present century (Schubert, 1974; Handberg, 1976). There were a number of early controversies about the merits of the psychometric model; however, it has now been incorporated as part of the information routinely transmitted to undergraduates at the level of abstract theory (e.g., Goldman and Jahnige, 1976) and also as the basis for empirical exercises (Ryan and Tate, 1975). Moreover, this model has been extended beyond the U.S. to encompass judicial decision making in a variety of cultural milieus and institutional settings (e.g., Blackshield, 1972; Schubert and Danelski, 1969). Despite the widespread acceptance and application of the psychometric model, there remains an indeterminacy in its procedures which heretofore has been solved by ad hoc approximation methods. The purpose of this article is to demonstrate that there is an exact mathematical solution to this indeterminacy and thereby to provide greater rigor in the psychometric modeling of judicial decision making.

Although I develop the particular solutions for this indeterminacy in the context of judicial decision making (because it was in this field that the problem was initially posed), the solutions proposed in this article are readily applicable to the more general problem of interpreting the underlying dimensions of multidimensional configurations derived from factor or smallest space analyses.

The Psychometric Model

The theory and methods of the psychometric model have now been explicated on a number of occasions (e.g., Schubert, 1965, 1974); however, I will provide here a brief overview of the relevant details before moving to the solutions for the indeterminacy that will become apparent.

The combination of attitudes of each justice toward issues raised and responded to in the stylized format of litigation are represented in a multidimensional space by an ideal (or *i*-) point. Each case before the court for decision is conceptualized as being a stimulus (or *j*-) point in that same space defined by the relevant attitudinal dimensions. Consequently, each manifest decision of the court is conceived as a function of the relationship between the *i*-point for each justice and the *j*-point. The dominance

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model (Coombs, 1964) specifies that if the intensity of the *j*-point exceeds or dominates the *i*-point of the relevant justice on the dimension under consideration, then a different result will emerge than when the *i*-point dominates the *j*-point. The dominance model provides the rationale for the cumulative scaling (or scalogram analysis) of judicial decisions. In the one-dimensional view of scalogram analysis, respondents will endorse any item (case) until the intensity of issues raised by a case exceeds the respondent justice's *i*-point. Thus both cases and justices are spatially represented by points in a space of a dimensionality determined by the variety of issues perceived by the justices to be raised in the cases before the court for decision.

From a methodological perspective the universe of cases for analysis in the psychometric model is subjected to two streams of somewhat independent analysis. On the basis of explicit coding rules concerning their hypothesized psychological content, cases are allocated to mutually exclusive attitudinal (rather than legal) categories representing C-scale issues of civil liberties, the E-scale of economic liberalism or, where appropriate, to other minor scales. These pools of cases are then separately tested for unidimensionality via Guttman's (1944) theory of a cumulative scale.

It has been found that the majority of cases decided by the United States Supreme Court are perceived to raise either civil libertarian or economic issues. An average of 75 percent of all non-unanimous cases decided by the Supreme Court between 1944 and 1968 adequately fit the C- and E-scales (Schubert, 1974, p. 64). This classification scheme stands in contrast to the way in which public lawyers insist on labeling cases as raising, say, First Amendment, Fifth Amendment and/or Fourteenth Amendment issues, on the presumption that such a framework provides the appropriate rubric for understanding judicial decision making.

The second stream of analysis utilizes all the cases, without regard for their hypothesized psychological content, as the observational basis for computing correlation coefficients of voting or opinion agreement (Schubert, 1965, 1969) for subsequent submission to a standard computer routine for multidimensional analysis. In his earlier studies Schubert used multiple factor analysis; however, with the development of nonmetric methods he has also used smallest space analysis (SSA)—a nonmetric multidimensional scaling algorithm developed by Louis Guttman (1968) and James C. Lingoes (1977).

Drawing upon relevant theories about the relationship between unidimensional cumulative scaling and multiple factor analysis (Coombs and Kao, 1960), Schubert used scalograms as an aid in interpreting the latent dimensions of the configuration of the justices' *i*-points. The constellation of ideal points is based upon the universe of cases that is subdivided on the basis of hypothesized psychological content to form the scalograms. Consequently, scalograms can aid in interpreting the underlying dimensions of interagreement expressed in the spatial arrangement of *i*-points in the factorial configuration. This procedure provides a solution to what in factor analysis is known as rotating the axes to a psychologically meaningful position: for Schubert the scalograms provide the criteria.

More precisely, Schubert's goal was to rotate a reference axis of the configuration to a position whereupon the projections of the justices' *i*-points yielded the same rank order as obtained from a cumulative scale. If such a criterion can be satisfied, then one has obtained an analogue of the cumulative scale in the multidimensional configuration (as evidenced by the identical or near-identical orderings of the justices) and may therefore interpret the configuration in terms of the known substantive content of that cumulative scale (Schubert, 1962; Rabinowitz, 1975).

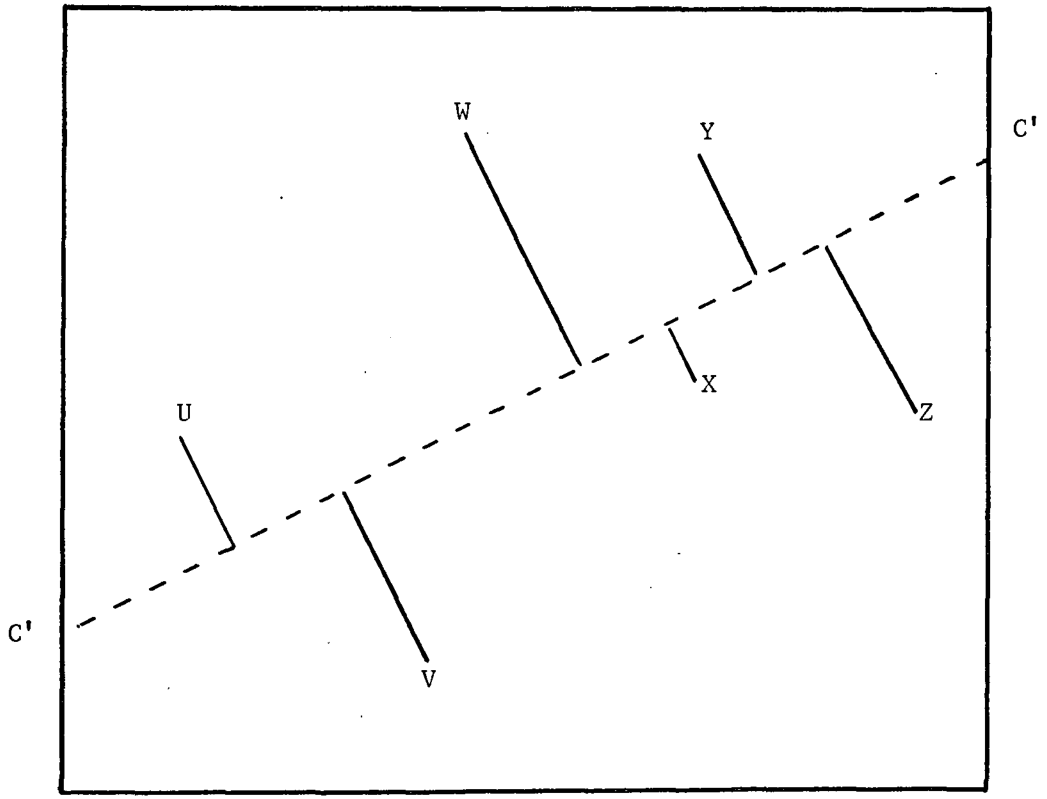
In a typical application of the psychometric model we wish to fit vectors representing the C- and E-scales through a multidimensional space. In empirical studies of the Supreme Court (Schubert, 1974) there are only a few instances in which perfect scale analogues have been obtained, and none in an easily visualizable two-dimensional space. Consequently for illustrative purposes Figure 1 provides an hypothetical two-dimensional constellation of *i*-points for six justices (U, V, W, X, Y and Z). The rank order of the same six justices on two hypothetical scalograms (labeled the C- and E-scales) is reported in Table 1.

Within the spatial configuration of Figure 1 a vector yielding the ordering of the justices on the C-scale can be found by inspection. This vector is represented by a broken line and its termini are signified by C′. The projections of the ideal points onto this vector faithfully reproduce the rank order of the C-scale. However, no such replication of the rank-order of the E-scale can be found. Under such circumstances we seek that location of the E-scale analogue which provides the "best" representation of its associated cumulative scale. It is a relatively trivial matter to determine whether a scale analogue can be located in a two-dimensional space. It is far more difficult to locate visually scale analogues within multidimensional spaces of dimensionality greater than two, or with the number of points typically found in legislative roll-call analyses.

The Indeterminacy of the Scale Analogues

The possibility of obtaining a mathematical solution for the problem of fitting scale analogues to a multidimensional configuration follows as a practical solution to the problem. Schubert (1965, p. 73) notes that a "best solution for the problem by setting a set of simultaneous equations has been explored, but I am informed by statisticians that the mathematical solution for the required set of equations is not known."

The only procedure presently available (and used by Schubert in his most recent application of the psychometric model) is to proceed heuristically. The configuration is visually inspected for an initial guess about the location of the scale analogues. The scale analogue is rotated after one compares the rank order of the projections upon the analogue with the rank order of the scalogram. The researcher intuitively estimates the direction and magnitude of these adjustments in order to bring about an improvement in the fit. This process continues until one either obtains a perfect fit or feels satisfied that the optimal solution (as indicated by some index of goodness of fit between the cumulative scale and its analogue) has been obtained. This manual procedure "guarantees neither that any acceptable solution will be found nor that if any acceptable solution is found, this will necessarily also be the best possible solution" (Schubert, 1974, p. 73). It is this apparent subjectivity, well recognized by Schubert, that forms the basis of S.



Source: Compiled by the author

Figure 1. Hypothetical Factorial Space of Six Justices

Sidney Ulmer's recent criticism (Ulmer, 1975, p. 56): a "more serious [problem] is the fact that the Psychometric model provides no method of determining the location of the inserted [scale analogue] vector in the space other than by approximation."[1]

Schubert's and Ulmer's sensitivity to the problem of determining the optimal location of scale analogues is well placed, given the importance of this procedure in the psychometric model. Identifying acceptable scale analogues provides strong evidence that the content of the cumulative scales represents the major attitudinal dimensions of agreement expressed in the configuration of ideal points. Thus we repose little confidence that the appropriate scale content has been identified if acceptable scale analogues cannot be found. On the other hand, if two scales consistently yield coincidental locations for their associated analogues, then the scales may well be combined into a single scale for a more parsimonious conceptualization of the underlying dimension of agreement. (The angular disparity between the C- and E-scale analogues, for example, provides a quantifiable index of the extent to which an unidimensional scale of liberalism-conservatism represents an oversimplification of multidimensional relations.) Thus identifying acceptable scale analogues imparts a degree of internal validity to the logic of the psychometric model.

This article outlines solutions for determining the location of the best-fitting scale analogues. In fact, two different solutions are developed reflecting different senses of "optimal," yielding, in turn, two different criteria to be optimized. These are the *least-squares* and *congruence* criteria. These two criteria may be further distinguished on the basis of whether the cumulative scale is treated at the level of interval (the metric case) or ordinal (the nonmetric case) measurement.[2]

The Metric Solutions

To facilitate the development of the mathematical solutions, it is convenient to recast Schubert's (1964) algebraic treatment of the problem into linear algebra. However, I will maintain the substantive context by addressing the problem in terms of fitting a cumulative scale to a three-dimensional factor analytic configuration of ideal-points of the nine justices of the U.S. Supreme Court. The generalization of the solution to dimensionalities greater (or less) than three, and to a different number of variables, is trivial once the required matrix equations have been developed.

Schubert (1965, p. 74) shows that for the three-dimensional case we seek weights, (α, β γ) for the dimensions, x, y and z, such that the projection of the ideal point of the i^{th} justice on the cumulative scale analogue, $\hat{p}_i$, is "close" to the original (cumulative) scale score of that justice, p_i. The projections on the scale analogue are algebraically defined as:

$$\hat{p}_i = \frac{\alpha x + \beta y + \gamma z}{(\alpha^2 + \beta^2 + \gamma^2)^{1/2}} ; \qquad (1)$$

where x, y and z represent the coordinate values of justice i on dimensions I, II and III; and α, β and γ are the required direction cosines that we seek. Equation (1) can be better understood if we note that the role of the denominator is to normalize the weights, α, β and γ, such that their sum of squares is equal to unity; therefore they have the property of direction cosines.

Casting the problem into linear algebra, using the example of Supreme Court justices, requires the creation of one matrix and a number of vectors (indicated respectively by upper and lower case letters). Defining X as a (n x m) [that is, (9 x 3)] matrix representing the coordinate values of the nine justice on three factorial dimensions yields

$$X = \begin{bmatrix} x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \\ \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \\ x_9 & y_9 & z_9 \end{bmatrix} .$$

θ is formed as a (3 x 1) column vector of direction cosines with elements represented by the Greek letters of equation (1); that is,

$$\theta = \begin{bmatrix} \alpha \\ \beta \\ \gamma \end{bmatrix} .$$

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[1] Flango and Ducat (1977, p. 45) have offered the same criticism of the psychometric model. It suffers "from subjectivity inherent in classifying cases for scalogram analysis and from the problem of equating factor loadings with cumulative scale scores." As a consequence they propose an orthogonal rotational scheme together with the use of factor scores.

[2] Fred Kort has developed a method for the solution of the problem when it is defined in terms of the interval (or metric) level of measurement. Kort's method employs Taylor's theorem.

Extending the scalars p_i and $\hat{p}_i$ over all 9 justices to form two (9 x 1) column vectors yields

$$p = \begin{bmatrix} p_1 \\ p_2 \\ \cdot \\ \cdot \\ \cdot \\ p_9 \end{bmatrix} ; \qquad \hat{p} = \begin{bmatrix} \hat{p}_1 \\ \hat{p}_2 \\ \cdot \\ \cdot \\ \cdot \\ \hat{p}_9 \end{bmatrix}$$

where p represents the Guttman scale scores, and $\hat{p}$ represent the estimates of p (that is, the projections on the scale analogue) under given values of the direction cosines.

We are now in a position to state succinctly equation (1), developed by Schubert, in matrix notation. The numerator of equation (1) becomes

$$\hat{p} = X\theta. \tag{2}$$

Rather than incorporating the denominator of equation (1) in equation (2), we state it as a side condition. That is, in solving for the vector of direction cosines in equation (2), θ *must* have the property that the sum of squares of its elements is equal to unity. Thus

$$\theta'\theta = 1. \tag{3}$$

The disparities between the cumulative scale scores, p, and the projections on the scale analogue, $\hat{p}$, are treated as a vector of error terms. Thus

$$e = p - \hat{p}. \tag{4}$$

Substituting for $\hat{p}$ from equation (2),

$$e = p - X\theta. \tag{5}$$

Consequently, the least-squares criterion (that is, the minimization of the sum of the squared disparities, $e'e$) yields the following function:

$$f_1 = (e'e) = (p - X\theta)'(p - X\theta). \tag{6}$$

The usual method of solving models of this form under side constraints, such as equation (3), is through Lagrange's undetermined multiplier, λ. This yields the following development to the problem of minimizing f_1 under the constraint of f_2, where equation (3) is rearranged to

$$f_2 = \theta'\theta - 1. \tag{7}$$

The function to be minimized, F, is

$$F = f_1 - \lambda f_2. \tag{8}$$

F is differentiated with respect to the elements of θ, set to zero, and after simplification

$$X'X\theta - \lambda\theta = X'p. \tag{9}$$

Equation (9) does not admit to further algebraic simplification if $m > 2$ since it is not linear in the unknowns θ and λ. Browne (1967) has developed a numerical scheme for the solution of equation (9) on the basis of the eigenvalues and eigenvectors of $X'X$. This leads to a set of equations which may be expressed in the form of one polynomial equation in one unknown. Consequently any of a number of iterative methods (such as Newton's) may be applied to solve for the roots of the equation. An adaptation of Browne's solution is outlined in the appendix. The imposition of the side-constraint leads to rather complex numerical methods for the required solution which, nonetheless, have been implemented in a FORTRAN coded computer program.

There are alternative criteria which may be optimized that are free of the extensive iterative techniques necessary for the least-squares solution. One such coefficient, the *congruence* criterion, has a long history in factor matching procedures (Harman, 1976, p. 343). This coefficient may be viewed as a correlation coefficient based on raw scores rather than deviations. Thus it reflects the degree of similarity between two columns and will obtain its maximum value even if the values of one column differ from the other by some constant of proportionality. In the present notation the congruence coefficient, c, is defined as:

$$c = \frac{(X\theta)'p}{[(X\theta)'(X\theta)]^{1/2}\,(p'p)^{1/2}}. \tag{10}$$

In order to maximize the congruence coefficient (Korth and Tucker, 1976), the vector θ may be taken as:[3]

$$\theta = (X'X)^{-1}X'p. \tag{11}$$

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[3] A number of researchers have utilized the procedure outlined here. Chang and Carroll (1969) have written a program, PRØFIT, which accommodates both interval and ordinal levels of measurement. Kruskal and Wish (1978) outline this procedure in the context of multidimensional scaling.

However, θ will not generally have the property that $\theta'\theta = 1$. Thus the associated direction cosines may be obtained by normalizing the vector θ. This normalization has no impact on the magnitude of the congruence coefficient.

The solution for the congruence criterion is of importance because equation (11) represents the usual solution for the coefficients in ordinary least-squares regression. By choosing equation (10) [rather than equation (6) under the side restriction of (3)] as the criterion to be optimized, we need no special software as the results may easily be obtained from a standard multiple regression routine. However, it will be necessary to normalize θ to obtain the direction cosines.

The Nonmetric Solutions

The above procedures provide solutions for the least-squares and congruence criteria. In both instances the cumulative scale scores of the justices are treated at the measurement level of interval values. This measurement level is one that Schubert (1965, p. 73) explicitly eschews as demanding too much confidence in the reliability of the data: "A [metric] least-squares solution, which only coincidentally results in maximal rank correlation, does not provide a "best" solution for the problem described. . . ." Nonetheless, metric solutions do provide good initial approximations for the required nonmetric solution: that *the rank order* of the orthogonal projections of the justices' *i*-points should be as "close as possible" to the rank order of the cumulative scale. This is a problem in nonmetric analysis and an iterative solution for the problem is easily obtained by combining principles drawn from algorithm construction in that area.

One way of viewing the problem in the nonmetric context is to allow any numerical value to be substituted for the cumulative scale scores as long as the substituted value maintains the rank order of the original scale scores. That is to say that we allow a class of potential values based upon monotonic (rank order-preserving) transformations of the original scale scores rather than the unique interval values required by the metric solution. Formally, the class of permissible monotonic transformations, $\mathcal{J}$, is

$$\mathcal{J}: \ (p_i < p_j) \rightarrow (p_i^* < p_j^*). \qquad (12)$$

This class of transformations allows monotonically transformed values p_i^*, for each cumulative scale score p_i. Thus for the least-squares nonmetric solution we require the *optimal monotonic* transformation within $\mathcal{J}$, and the optimal direction cosines, θ, such that

$$g_1 = (p^* - X\theta)'(p^* - X\theta) \qquad (13)$$

is a minimum. Note that in minimizing g_1 it is still necessary to impose the side constraint that $\theta'\theta = 1$.

The function to be maximized for the nonmetric variant of the congruence coefficient is:

$$g_2 = \frac{(X\theta)'p^*}{[(X\theta)'(X\theta)]^{1/2} \, (p^{*\prime}p^*)^{1/2}} \, . \qquad (14)$$

For both the least-squares and congruence criterion there is a marked similarity between the metric and nonmetric functions. In addition to their solution for the direction cosines, the nonmetric procedures also require the optimal monotonic transformation of the cumulative scale scores, p^*, for substitution in equations (13) and (14). The metric solution does not allow monotonic transformations but rather treats $p^* = p$ in these equations.

The Nonmetric Algorithms

The above developments specify the required *definitional* aspects for the construction of algorithms for the nonmetric solutions. Although the nonmetric algorithms were designed before the appearance of important new developments in nonmetric analysis (de Leeuw, et al., 1976; Young et al., 1976), they can be seen as falling within the class of alternating least-squares algorithms. This framework provides an intuitive understanding of the algorithms' operational features.

The alternating least-squares approach decomposes a problem into several (two in the present case) subproblems or phases. By sequentially alternating between these two phases, both of which are least-squares solutions for their respective subproblems, we can obtain the required overall solution. The first phase consists of determining the least-squares monotonic transformation of the cumulative scale scores given some conditional estimate of the direction cosines, θ. The second phase determines the least-squares solution for the direction cosines given some (conditional) monotonic transformation within $\mathcal{J}$. By moving back and forth between these two phases we consistently improve the overall goodness of fit until no further improvement is possible.

In practice the two phases are estimated in the following fashion: In the first phase we desire a monotonic transformation of the cumulative scale scores. Within the literature on nonmetric analysis there are two widely accepted procedures that can be used here for this purpose. The first method is Kruskal's (1964) *monotone* regression procedure; the second is Guttman's (1968) rank-image transformation.[4] To maintain strict fidelity to the requirements of the alternating least-squares approach, Kruskal's algorithm should be the choice because it yields a *least-squares* monotone transformation. However from a practical standpoint there is very little difference between the results of the two procedures (Spence, 1972; Lingoes and Roskam, 1973).

The second phase is designed to estimate the least-squares values for the direction cosines given some conditional estimate of the transformed values, p^*. This phase is implemented through the application of the usual metric procedure outlined previously and detailed in the appendix. It is of no conceptual significance that the second phase entails iterations within its own procedure. All that is required of this phase is that it returns the required least-squares solution, given some monotone-preserving estimate of the cumulative scale scores.

At the commencement of the iterative process for the nonmetric solution we do not have estimates of either the monotonic transformation or the direction cosines. To provide starting values that are reasonably close to the final yet unknown values a metric *least-squares* solution is obtained. This initial solution not only saves computer time but also provides some protection against convergence to a local rather than global minimum of the function. Moreover, it allows the user to evaluate the gains made from using a nonmetric criterion rather than the metric values. In summary, on the first iteration the transformed values and the original cumulative scale scores are equal. The first *iteration* is completed after the second phase is invoked and the direction cosines are computed from these values. The first phase of the next iteration consists of applying a monotonic transformation to the current estimate of p^* using either Kruskal's or Guttman's procedure. On each application of this procedure we obtain, by construction, a set of monotonically transformed values of the original cumulative scale scores. Using this set of new estimates of p, we obtain new and improved estimates for the direction cosines. This completes another iteration and the iterative process is continued until convergence is reached and the application of the two-phased procedure evokes no further improvement in the function to be minimized. At this point both the optimal monotonic transformation of the original cumulative scale scores and the optimal direction cosines have been obtained. Consequently the nonmetric least-squares solution for the fitting scale analogues has been derived.

The only disparity between the solutions proposed here and the hand procedure developed by Schubert is that the present algorithms do not attempt to maximize the relationship between the cumulative scale and its analogue as measured by Spearman's *rho* coefficient. Rather, alternative coefficients of monotonicity which are more amenable to algorithm construction are the goal of the optimization process. There is of course, no compelling reason why maximizing *rho*, or *tau*, or any other coefficient of monotonicity between the cumulative scale and its analogue will necessarily yield identical locations for the scale analogue. What does seem clear is that the analogues will be located in proximate areas of the multidimensional space.

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[4] An excellent introduction to these two approaches has been developed by Rabinowitz (1975). In order to provide some meaning to the idea of a monotonic transformation I will describe here the rank-image transformation as it is simpler than Kruskal's (1964) transformation. Consistent with the text, the projections on the scale analogue are denoted as $\hat{p}$ and the cumulative scale scores as p. In order to construct the vector of rank images the projections, $\hat{p}$, are compared to the scale scores. Any elements of $\hat{p}$ that are not in the same rank order as the cumulative scale scores are permuted to that order. This yields a vector, p^*, containing the same numerical values as $\hat{p}$ but in the rank order of p. By construction the rank order of the elements of p^* is the same as that of p; hence we have applied a monotonic transformation to p. The vector of permuted values, p^*, are known as the rank images. Obviously if a perfect solution has been obtained, then no permutation is necessary as $p \overset{m}{=} \hat{p} = p^*$ (where $\overset{m}{=}$ denotes "is monotone with" or "is in the same rank order as"). Note that *only* those values in $\hat{p}$ which are not in the same rank order as p are permuted. It can be seen that the rank images represent a set of "intermediate" values, having the numerical values of the projections but the rank order of the cumulative scale scores. On the next iteration the rank images represent the estimates of the monotonically transformed cumulative scale scores.

The computer programs that have been developed use the rank image transformation; however, rank images differ only slightly (at convergence) with the values derived from Kruskal's transformation. The difference is unlikely to be of practical significance.

The description of the development of the nonmetric algorithms applies only to the least-squares version. However, very similar principles apply to the construction of the nonmetric congruence algorithm.

Applications

The theoretical developments provided above explicate an appropriate procedure for fitting scale analogues to a configuration of points of any dimensionality. The most satisfactory *practical* procedure for testing the algorithms is to compare their results to those obtained from independent analyses. For this purpose FORTRAN-coded computer programs were developed to implement metric and nonmetric versions of the least squares (NOLS) and congruence algorithms (NOCON) that have been described above. The data sets that form the basis for this comparison were arbitrarily selected as the initial dozen solutions for fitting scale analogues to the principal components solutions that Schubert reports in *The Judicial Mind Revisited.* For each of the initial six terms of his study Schubert fitted both the C- and E-scale analogues to these constellations and the 12 *rho* rank-order correlations he obtained are reported in Table 2.

Table 1. Hypothetical Ranks for Six Justices on the C- and E-Scales

Rank-Order	C-Scale	E-Scale
1	U	X
2	V	Z
3	W	U
4	X	W
5	Y	V
6	Z	Y

Source: Compiled by the author.

It seems apparent that the programs should do as well or better than Schubert's hand procedures for the data sets under analysis. However, this is not necessarily the case for two reasons. First, Schubert's goal was to maximize Spearman's *rho* coefficient whereas the programs were designed to optimize similar, but not identical, monotonicity coefficients. In order to overcome this difficulty and provide a more direct basis for comparison I modified the programs to compute and to maintain a record of that solution encountered during the iterative process that yielded the highest *rho* coefficient. It is this solution that forms the basis for the comparison. There is of course, no guarantee that in the course of iterations to optimize a function the programs will encounter the best theoretically obtainable *rho* solution. Indeed in 7 of the 12 solutions the NOLS program continued to iterate after the

Table 2. A Comparison between the Results Obtained by Schubert and Those Derived by Analytical Means

Deck	Scale	Nonmetric Congruence Coefficient[a]	Nonmetric Least-Squares Criterion[b]	Schubert's Results
1	C	0.9958	0.9833	1.000*
	E†	0.9542	0.9458	0.970*
2	C	0.9667*	0.9333	0.900
	E	0.8833**	0.8833**	0.867
3	C†	0.9792**	0.9792**	0.954
	E††	0.9917**	0.9917**	0.966
4	C	1.0000**	1.0000**	1.000**
	E	0.9833**	0.9833**	0.983**
5	C†	0.8833**	0.8833**	0.881
	E†††	0.9708	0.9708	0.992*
6	C†	0.9792	0.9708	0.996*
	E	0.9667	0.9883*	0.967

Source: Data compiled by the author on the basis of his calculations and from Glendon Schubert (1974), *The Judicial Mind Revisited,* New York: Oxford University Press, p. 74.

*The best solution for each analysis.

**The highest *rho* coefficient was obtained by two or more procedures.

†The presence and number of tied ranks in the cumulative scales. The mean of the ranks was used when computing the *rho* coefficient in the presence of tied ranks on the cumulative scales.

[a] The results for the nonmetric congruence coefficient were obtained by the NOCON program.

[b] The results for the nonmetric least-squares criterion were obtained by the NOLS program.

"maximum *rho*" solution had been obtained because this solution was suboptimal when evaluated in terms of the coefficient that the program was designed to optimize. Consequently, a slightly lower *rho* coefficient was associated with the final solution than with some of the intermediate solutions the program obtained.

The second source of difficulty is the occurrence of ties both in the cumulative scale ranks and also in the projections of the points onto the fitted vector. The programs work to the full precision of the computer–about 7 decimal places–even though the input data included numbers significant to 3 decimal places. In the first data set analyzed the NOLS program faithfully reproduced the required rank-order of the cumulative scale except for one reversal in ranks where the difference between the two relevant projections was *less than .0001.* From a practical standpoint this represents a tied projection and the *rho* coefficient should be adjusted upward accordingly. However, no such adjustments were made as it is not clear from Schubert's discussion how ties (either in the cumulative scale or in the projections on the fitted vector) were treated.[5] Therefore I assigned equal values to tied rankings on the cumulative scale, and under these circumstances a perfect correlation could only be obtained if the projections on the scale analogue were also tied to the full precision of the computer.

Table 2 reports the results of the comparison for fitting the C- and E-scale analogues to the initial 6 principal component configurations. This yields a total of 12 solutions. In 4 instances Schubert obtained higher *rho* coefficients than were obtained by the NOLS program; in contrast, NOLS obtained better *rho* solutions than Schubert's results in 6 instances, while for two analyses the *rho* coefficients for the two procedures were equal. Schubert's hand results were also superior to the NOCON program on 4 analyses; and the tables reversed 5 times. NOCON yielded higher *rho*'s than NOLS on 4 occasions and only once were the roles reversed.

Even though the programs were not designed to maximize the *rho* coefficient, their performance was superior to Schubert's hand approach to the problem. As 6 of the 12 analyses were ones where ties occurred in the cumulative scale rankings, it can be seen that the bases for the comparison were biased against the superior performance of the computer programs.

Conclusions

The mathematical solutions for fitting scale analogues to multidimensional configurations have been provided for the metric case as well as extended to the nonmetric case. These solutions (and the accompanying programs) remove the criticism leveled at the psychometric model that locating scale analogues is subjective. The computer programs optimize clearly defined objective functions and consequently outperform hand analyses carried out on the basis of visual inspections. These same procedures can be applied to problems of greater complexity than 9 points in 3 or 4 dimensions of the psychometric model of the U.S. Supreme Court.

The implications of the reanalysis for the substantive findings reported in *The Judicial Mind Revisited* are straightforward. Although half of the 12 solutions derived from the programs yielded solutions with higher *rho* coefficients than those reported by Schubert, these increments average 0.03, with the largest improvement being 0.07. These improvements in the magnitude of the *rho* coefficients indicate that Schubert's results were often suboptimal. However, the revised coefficients provide *greater* support for the interpretation he provides. Presumably further gains could be achieved with a more practical decision rule for the treatment of ties than the conservative test that I have chosen which virtually eliminates the possibility of a perfect correlation if ties occur in a cumulative scale.

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[5] Although the option was not exercised for reasons discussed in the text, ties can be routinely treated in both programs through the use of a "slack" factor parameter. The slack factor specifies a band within which values are treated as ties. For example, if the slack factor is set at 0.050 (a value considered by Schubert, 1965, p. 82) then projections would be considered as tied when the difference between them is less than this amount.

The programs also have the capability of allowing the user to specify estimates of the direction cosines. By setting the maximum number of iterations to one, the user obtains the projections and the associated *rho* coefficient for that transformation. If the user allows the programs to iterate, the the specified values are used as initial estimates. This provides a solution to the local optima problem that pervades most iterative algorithms by providing for different starting values. In some instances different starting values yielded a higher "maximum *rho*" solution because the program moves to the optimal solution along a different track. All the results reported in these analyses are based on the solutions obtained from the internally generated starting values of the programs.

There were two periods for which "unacceptable" fits are reported by Schubert (where "unacceptable" is taken to mean *rho* coefficients less than 0.90). In these cases, as Table 2 shows, the programs provided only marginal improvements–from 0.867 to 0.883 for the deck 2 E-scale and from 0.881 to 0.883 for the deck 5 C-scale. These results still fall just short of the requisite benchmark for acceptance. However, we can conclude that these results are not so much a function of Schubert's failure to find the optimal location for the scale analogues but rather seem dictated by the attributes of the data sets.

Consistently with Schubert's earlier remarks, neither the proposed method of interpreting configurations nor the mathematical solutions are procedures limited in applicability to the psychometric model of judicial decision making. It is, rather, a general procedure for interpreting multidimensional configurations with the aid of an external criterion. As such, the procedure falls within the class of rotational criteria known as procrustes rotation; that is, transforming the configuration to best fit with a target. The unique aspect of the problem as posed by Schubert, is that the target is defined only up to the ordinal (or nonmetric) level of measurement. Certainly this is more consistent with the level of measurement generally associated with the behavioral sciences.

Appendix

Recall that our problem is to re-arrange equation (9) in the text to a more tractable form in order to solve for the direction cosines, θ. Equation 9 may be written as:

$$X'X\theta - \theta\lambda = X'p. \qquad (1)$$

We can reduce $(X'X)$ to diagonal form by computing the eigenvalues and eigenvectors of the $(m \times m)$ matrix product. Let D be the $(m \times m)$ diagonal matrix of eigenvalues and U the associated eigenvectors normalized to unit length. Thus

$$U'X'XU = D \qquad (2)$$

where

$$U'U = UU' = I. \qquad (3)$$

Defining a new $(m \times 1)$ vector, w, as

$$w = U'\theta; \qquad (4a)$$

it follows that

$$\theta = Uw. \qquad (4b)$$

Premultiplying equation (1) by U'

$$U'X'X\theta - U'\theta\lambda = U'X'p \qquad (5)$$

and substituting (4b) in the first and (4a) in the second term on the left

$$U'X'XUw - w\lambda = U'X'p \qquad (6)$$

and substituting equation (2) for the first term on the left,

$$Dw - w\lambda = U'X'p. \qquad (7)$$

For convenience we define a new $m \times 1$ vector, x, such that $x = U'X'p$, and substitute in equation (7)

$$Dw - w\lambda = x. \qquad (8)$$

In scalar form equation (8) can be written as:

$$\begin{aligned} d_1 w_1 - w_1\lambda &= x_1 \\ d_2 w_2 - w_2\lambda &= x_2 \\ \ldots \quad \ldots \quad &\ldots \\ d_m w_m - w_m\lambda &= x_m. \end{aligned} \qquad (9)$$

Rearranging terms leads to

$$w_i = \frac{x_i}{d_i - \lambda} \quad (i = 1, \ldots, m). \qquad (10)$$

Recall that $w'w = 1$ because from equation (4a) $w'w = \theta'UU'\theta = \theta'I\theta = \theta'\theta = 1$. In scalar form,

$$\sum_{i=1}^{m} w_i^2 - 1 = 0. \qquad (11)$$

The m equations of (10) can be combined with (11) to yield $(m + 1)$ unknowns, λ, w_1, $w_2, \ldots, w_m$. The resulting $m + 1$ equations can be expressed as one equation in one unknown, λ. Substituting equation (10) into (11) yields

$f(\lambda) = 0$, where

$$f(\lambda) = \sum_{i=1}^{m} \frac{x_i^2}{(d_1 - \lambda)^2} - 1. \qquad (12)$$

Browne (1967) shows that the required absolute minimum for the least-squares solution corresponds to the smallest real root of $f(\lambda)$. Moreover, this root is less than the smallest eigenvalue of $(X'X)$. Now the roots of (12) cannot be solved by algebraic means; however, iterative numerical methods (such as Newton's) may be used. The roots of this polynomial equation can be solved by either first or second derivative means. Browne uses the second derivatives; however, that approach adds additional complexity.

If $\lambda^{(k)}$ is an approximation of a root of $f(\lambda)$ on the k^{th} iteration, then an improved approximation $\lambda^{(k+1)}$ of the root is given by

$$\lambda^{(k+1)} = \lambda^{(k)} - h^{(k)} \qquad (13)$$

$$\text{where } h^{(k)} = \left[\frac{f(\lambda)}{f'(\lambda)}\right]_{\lambda=\lambda^{(k)}};$$

$$f'(\lambda) = 2\sum_{i=1}^{m} \frac{x_i^2}{(d_i-\lambda)^3},$$

and

$$f(\lambda) = \sum_{i=1}^{m} \frac{x_i^2}{(d_i-\lambda)^2} - 1.$$

A generally satisfactory initial approximation to the smallest root of $f(\lambda)$ is one-half of the smallest eigenvalue of $(X'X)$. If in the course of the iterations implied by equation (13) the value of $\lambda^{(k+1)}$ exceeds the smallest eigenvalue, r, of $X'X$, then $\lambda^{(k+1)}$ should be replaced by $(\lambda^{(k)} + r)/2$.

This procedure is continued until convergence and $f(\lambda^{(k)})$ is close to zero. The elements of w in equation (10) are obtained from equation (4b). That is, the elements of w are premultiplied by the matrix of normalized eigenvectors of $(X'X)$.

Typically only 4 or 5 Newton iterations are required for convergence, and consequently the procedure takes only two or three seconds on a medium-sized computer.

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Religious vs. Linguistic vs. Class Voting: The "Crucial Experiment" of Comparing Belgium, Canada, South Africa, and Switzerland*

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For the purpose of determining the relative influence of the three potentially most important social and demographic factors on party choice—social class, religion, and language—a comparison of Belgium, Canada, South Africa, and Switzerland provides a "crucial experiment," because these three variables are simultaneously present in all four countries. Building on the major earlier research achievements in comparative electoral behavior, this four-country multivariate analysis compares the indices of voting and the party choice "trees" on the basis of national sample surveys conducted in the 1970s. From this crucial contest among the three determinants of party choice, religion emerges as the victor, language as a strong runner-up, and class as a distant third. The surprising strength of the religious factor can be explained in terms of the "freezing" of past conflict dimensions in the party system and the presence of alternative, regional-federal, structures for the expression of linguistic interests.

The major overall conclusion which emerges from comparative studies of the social and demographic bases of voting behavior is that there are two especially important determinants of party choice: social class and religion. Compared to these two strong factors, the influence of other social and demographic variables tends to be much weaker. However, a third potentially powerful variable—language—has received scant attention so far. The main reason for this undeserved neglect appears to be that linguistic cleavages are relatively rare in Western democracies.

There are several Western countries with small linguistic minorities, but only three that can really be said to be linguistically divided: Belgium, Canada, and Switzerland. Hence these three countries are obvious candidates for comparison with regard to the impact of linguistic divisions on voting (see Hill, 1974, p. 100). An interesting fourth case that can be added is the Republic of South Africa. This country—or, more precisely, its white electorate—fits all three of the theoretical criteria used by Richard Rose (1974b, pp. 5–6) to demarcate the universe of countries suitable for inclusion in his comparative handbook on *Electoral Behavior:* the persistence of competitive free elections since 1945, a high rank on the conventional socioeconomic indicators of industrialization, and Christian cultural origins.

Another reason why these four countries are especially interesting is that they present an outstanding opportunity for a multivariate analysis of the *relative* weights of the linguistic, religious, and class variables, since all four countries are divided along all of these dimensions. In addition to their linguistic cleavages and the socioeconomic or class differences present in all industrialized societies, there is a Protestant-Catholic division in Canada, South Africa, and Switzerland, and a religious-secular split in homogeneously Catholic Belgium. The latter cleavage is at least as important as the

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denominational difference in the other three countries. In fact, the analysis of Belgian social and political structure in terms of the three cleavages of religiosity, class, and language can be said to have become the standard approach (Lorwin, 1966; Urwin, 1970; Huyse, 1975; Frognier, 1976).[1]

This study will use recent survey data—all collected in the 1970s—for a systematic comparison of the influence of language, religion, and class on party choice in Belgium, Canada, South Africa, and Switzerland. It is explicitly designed to be a cumulative effort and to build onto such prior research achievements in comparative voting behavior as Rose's handbook (1974a) and Robert R. Alford's analysis (1963) of class voting in the Anglo-American democracies. The methods and indices that have proved valuable in these earlier studies will be used here again: in particular, the AID (automatic interaction detector) technique, also known as tree analysis, which is the major multivariate statistical method employed in the Rose volume, and Alford's well-known index of class voting and similar indices for measuring linguistic and religious voting.

Alternative Comparative Strategies: The Value of a "Crucial Experiment"

Religion and social class have been recognized as prime determinants of party choice from the very beginning of comparative voting behavior research, but no consensus has emerged about which of the two variables is the *better* predictor. The two earliest cross-national analyses reached diametrically opposite conclusions. Seymour Martin Lipset (1960, pp. 220, 223–24) found that although religious differences could also contribute to party support, "on a world scale, the principal generalization which can be made is that parties are primarily based on either the lower classes or the middle and upper classes." And he added: "More than anything else the party struggle is a conflict among classes, and the most impressive single fact about political party support is that in virtually every economically developed country the lower-income groups vote mainly for parties of the left, while the higher-income groups vote mainly for parties of the right." But J. J. de Jong (1956, p. 160)—in what was probably the very first broadly comparative analysis of voting behavior, based on survey results from eleven European countries—concluded that religion was of primary significance and that class, although important, occupied a lower place in the hierarchy of voting determinants: "For the explanation of electoral behavior both in our own country [the Netherlands] and abroad, we always have to pay attention first to the religious factor."

The evidence on which these early generalizations were based was fragmentary and of uneven quality. Later and more systematic cross-national analyses that could benefit from more and better survey findings, have not been able to resolve the disagreement although they have been considerably more cautious in stating their conclusions. Two cross-national analyses, of four and eight countries, respectively, find that class voting tends to be stronger than religious voting (Alford, 1963; Budge and Farlie, 1976), but two other comparative studies, of fourteen and ten nations, respectively, conclude that religion tends to be the stronger factor (Rose and Urwin, 1969; Lijphart, 1971).[2] A fifth attempt to compare the relative influence of class and religion on party choice in twelve countries finds the two variables to be of about equal strength (Lancelot, 1975). These studies differ in several ways, but the critically important explanation of their divergent findings is that they are based on very different samples of countries: the religious factor is especially strong in the continental European countries with the exception of Scandinavia, and the proportion of these core continental European states in the different samples is highly unequal, ranging from zero percent in Alford's study to 60 percent in Lijphart's. The percentages are in perfect agreement with the conclusions of the five studies: zero percent and 25 percent, respectively, in the Alford and Budge-Farlie analyses that emphasize social class as the stronger variable, 33 percent in Lancelot's "neutral" study, and 43 and 60 percent, respectively, in the Rose-Urwin and Lijphart studies that find religion to be the better predictor.

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[1] Another reason for the comparative analysis of these four countries is that they have been rather neglected by students of voting behavior. Even Rose's (1974a) ambitiously wide-ranging volume treats only two of them: Belgium and Canada. And the chapter on Belgium in the Rose handbook is seriously handicapped by the absence of a question on language in the 1968 national election survey on which most of its findings are based.

[2] Rose and Urwin (1969) actually present findings on 17 countries, but data on the religious basis of party choice are lacking in two cases (Denmark and Finland), and one case is not a "country" on a par with the other sovereign states (Northern Ireland).

What are the reasons for this wide variation in the composition of the samples? Alford (1963, p. 4) chose his four countries, all of them outside continental Europe, because they are comparable cases: "The common political culture of the Anglo-American countries means that a comparative study of political cleavages is not complicated by widely varying political values and traditions." The other authors applied a looser standard of comparability and tried to maximize the size of their samples by including all Western countries with Christian traditions on which appropriate data were available, but Budge and Farlie also included Japan.

In order to arrive at a definitive conclusion about the relative influence of class and religion on party choice, one might take either of two alternative approaches. One approach would be to attempt to maximize the sample to such an extent that it would closely approximate or, ideally, coincide with the universe of countries with free electoral competition. The disadvantage of this strategy is that, while it appears to be impartial, it actually gives social class a better chance to emerge as the more significant predictor: class cleavages are present and politically salient to at least some extent everywhere, whereas denominational differences may be absent and differences in religiosity, though in principle present everywhere, are politically relevant mainly in the continental European countries with their traditions of religious-anticlerical conflict.

The logical alternative is therefore to focus the analysis on those countries where the forces of class and religion actually compete with each other. These cases may be labeled "comparable," not in the usual sense of being similar in a large number of background characteristics and dissimilar with respect to the operative variables (see Lijphart, 1975), but in the sense that all of the variables which the researcher tries to relate to each other are present—and hence truly operative—in all cases. This strategy resembles what Raoul Naroll (1966, p. 336) calls a "crucial experiment," in which cases may be selected "because certain variables happen to be present together." Philip E. Converse is thinking in terms of this alternative approach when he states: "The general rule seems to be that religious differentiation intrudes on partisan political alignments in unexpectedly powerful degree *wherever it conceivably can*" (1974, p. 734, emphasis added). Similarly, Giovanni Sartori argues that class may have a strong influence on party choice only when there are no powerful competing influences, and that therefore "the correct formulation of the problem is: *Given a multiplicity of cleavages*, can it be shown that there is a hierarchy of cleavages according to which the class cleavage tends to prevail?" (1969, p. 76, emphasis added.)

The contrast between the two approaches is even more striking when language is added as a possible predictor of party choice, because linguistic differences are relatively rare. Rose and Urwin (1969, p. 14) exemplify the first approach when they conclude that "linguistic or ethnic communalism is not today a major source of social cohesion for parties in Western countries." This conclusion is entirely legitimate and empirically correct for the universe of countries with free elections. But although linguistic and ethnic heterogeneity is rather infrequent, "where it occurs, . . . it seems to become expressed in partisan cleavages as persistently and vigorously as religious differentiation" (Converse, 1974, p. 735). This study adopts the perspective exemplified by the latter statement: it is a "crucial experiment" of the relative impact of language, religion, and class on party choice in the four countries where all of these variables are simultaneously present.

Four Multidimensional Multiparty Systems

In order to compute Alford's index of class voting, one must divide the voters into manual and non-manual workers and the political parties into left and right parties.[3] Similarly, the calculation of the indices of religious and linguistic voting requires the dichotomous division of the voters into Protestants and Catholics, frequent churchgoers (attending at least twice a month) and those attending church only infrequently or never, and people speaking the majority language vs. those speaking the minority language, as well as the dichotomization of the parties along the Protestant-Catholic, religious-secular, and linguistic dimensions. The classification of the respondents presents few problems. The only difficulty worth noting is that multilingual Switzerland is treated as bilingual, but among the Swiss voting population the Italian-speakers and Romansh-speakers constitute a minority of only about five percent, and it is therefore permissible to focus exclusively on the German-French dichotomy. Further details about the operational definition of the variables and the characteristics of the survey data are given in the appendix.

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[3] The following rule is used to compute the index of class voting: "Subtract the percentage of persons in non-manual occupations voting for Left parties from the percentage of persons in manual occupations voting for Left parties" (Alford, 1963, pp. 79–80).

Table 1 presents the classification of the political parties in the four multiparty systems along the left-right, Catholic-Protestant, religious-secular, and majority-minority language dimensions.[4] Many of the parties can be classified easily on the basis of their programs and traditions. For instance, as far as the left-right cleavage is concerned, the Belgian and Swiss Socialists and the Canadian New Democratic Party (NDP) clearly belong to the left, and the Belgian Liberals, the Swiss Radicals and Liberals, and the Canadian Conservatives to the right. With regard to the two religious dimensions, the Belgian Christian Social party and the Swiss Christian Conservative and Christian Social parties obviously fit the Catholic and religious categories, whereas the Belgian and Swiss Socialists, Radicals, and Liberals are traditionally secular parties. The linguistic cleavage divides the Flemish Volksunie (VU) from the Brussels Francophone party (FDF) and Walloon Rally (RW) in Belgium; similarly, the National party (NP) and its extremist offshoot, the Reconstituted National party (HNP), are Afrikaner parties of linguistic-ethnic defense. The smaller Swiss parties, which are regionally concentrated, can also be assigned easily to a linguistic category: the Liberals, who are particularly strong in francophone Switzerland, the Farmers (PAB) with special strength in Berne, and the mainly Zurich-based Independents.

In more doubtful cases, the criterion of classification is the inductive one of maximizing the indices of class, religious, and linguistic voting. The results contain no great surprises or anomalies. The only possibly controversial classifications in Table 1 are the left-right divisions in Canada and South Africa. The inductively drawn Canadian dichotomy that yields the highest index of class voting, interchanges the Liberals and Social Credit (SC) in Alford's left-right classification of Liberals and NDP vs. Conservatives and Social Credit. However, it is just as reasonable to assign the basically centrist Liberals to the right as to the left, and, although most scholars place Social Credit on the extreme right, the voters feel much less certain about this matter (Elkins, 1974, pp. 502–11; Laponce, 1970, pp. 483–85). For South Africa, the placement of the Nationalists and HNP on the left and the United (UP) and Progressive (PP) parties on the right reverses the usual classification of these parties on the liberalism-conservatism scale, but the latter classification is based on attitudes toward ethnic and racial issues rather than class issues (Peele and Morse, 1974, pp. 1532–34). In terms of class-based support, the Nationalists have traditionally received more lower-class votes than their opponents (see van der Merwe and Buitendag, 1973, pp. 201–04).[5]

With regard to the party dichotomies on the two religious dimensions, it should be noted that the dividing lines coincide in all three countries in which both a Catholic-Protestant and a religious-secular split occurs, but that the descriptive labels differ: the "Catholic" parties are also the "religious" parties in Canada and Switzerland, but they are the "secular" parties in South Africa. Finally, the linguistic dichotomy is identical with the religious dichotomies in all countries except Switzerland.

Class Voting, Religious Voting, and Linguistic Voting

The indices of class, religious, and linguistic voting for the four countries are presented in Table 2. The top half of the table contains the unadjusted (uncontrolled) indices that are basically the same measures as are used by Alford. The adjusted indices of voting in the lower half represent the "pure" voting index of a particular independent variable when the other independent variables are controlled.[6]

The most striking result that emerges from the array of indices is the weakness of class voting. In all four countries, the unadjusted indices of religious and linguistic voting all reach higher levels. The only relatively high index of class voting is the Swiss index of +21,

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[4] Only the more important parties are taken into consideration, and the very small parties that received less than roughly three percent of the votes in the elections of the early 1970s are disregarded.

[5] In recent years, the South African United and Progressive parties have regrouped: the left wing of the United party has merged with the Progressives to form the Progressive Federal party; its center has combined with the small Democratic party into the New Republic party; and its right wing continues as the South African party.

[6] Alford's indices of voting, which are differences in proportions, can also be interpreted as the regression coefficients for the regression of party choice on the independent variables if these dichotomized variables are given the numerical values of 0 and 1 (Alker, 1965, pp. 59, 84–85; Särlvik, 1969, p. 132; Korpi, 1972, p. 631). This important characteristic of the index makes it possible to use multiple regression analysis to compute controlled indices of voting.

Table 1. Classification of 21 Parties in Four Countries According to Four Dimensions of Cleavage

	Class Dimension		Religious Dimension I		Religious Dimension II		Linguistic Dimension*	
	Left	Right	Protestant	Catholic	Secular	Religious	Majority	Minority
Belgium	Socialist	Christian Social Liberal Volksunie Walloon Rally Francophone Front	–	–	Socialist Liberal Walloon Rally Francophone Front	Christian Social Volksunie	Christian Social Volksunie	Socialist Liberal Walloon Rally Francophone Front
Canada	New Democratic Social Credit	Liberal Conservative	Conservative New Democratic	Liberal Social Credit	Conservative New Democratic	Liberal Social Credit	Conservative New Democratic	Liberal Social Credit
South Africa	National Reconstituted National	United Progressive	National Reconstituted National	United Progressive	United Progressive	National Reconstituted National	National Reconstituted National	United Progressive
Switzerland	Socialist	Christian Conservative Christian Social Radical Liberal Farmers Independent	Socialist Radical Liberal Farmers Independent	Christian Conservative Christian Social	Socialist Radical Liberal Farmers Independent	Christian Conservative Christian Social	Christian Conservative Christian Social Farmers Independent	Socialist Radical Liberal

Source: Classification by the author on the basis of the parties' programs and traditions and the maximization of the indices of class, religious, and linguistic voting. The majority and minority languages are, respectively: Dutch and French (Belgium), English and French (Canada), Afrikaans and English (South Africa), and German and French (Switzerland).

but even here the other indices surpass it.[7] When the indices of class voting are adjusted for the influence of church affiliation, church attendance, and language, they undergo only a slight further weakening, or none at all, but the other adjusted indices remain stronger with the exception of the two religious voting indices in South Africa and the Canadian index based on church attendance.

Whereas it is easy to identify class as the weakest variable, it is much more difficult to say which is the strongest of the independent variables. Three different patterns appear among the unadjusted indices: religion as the strongest factor (Switzerland), language as the strongest factor (South Africa), and religion and language with approximately equal strength (Belgium and Canada). The Belgian and Canadian patterns fit the third category, but they are not exactly alike: in each of the countries, one of the religious variables and language are almost equally strong, but the strong religious variable is church attendance in the former case and church affiliation in the latter.

The adjusted indices of religious and linguistic voting differ considerably from the unadjusted indices, but the overall patterns remain the same for three of the four countries. The Belgian indices both decrease in value by about equal amounts, and hence they remain close together. The predominance of language in South Africa is accentuated by the introduction of controls: the index of linguistic voting stays at its very high level whereas the other indices are reduced to insignificance. This finding for South Africa as a whole confirms the earlier conclusion by Stanton Peele and Stanley J. Morse (1974, p. 1537), based on a voting study of three Cape Town constituencies, that "ethnicity [or language] is the major determinant of party vote. . . . Party vote also varies with other demographic categories (SES, religion, place of origin), but this is largely because these factors covary with ethnicity. When ethnicity is held constant, therefore, all these variables lose their predictive power." In Switzerland, church affiliation remains the most potent variable, but the difference with the linguistic voting index has narrowed. Also, church attendance and language are now at the same level. In Canada, finally, the pattern of equal religious and linguistic indices is changed by the introduction of controls. In line with the findings of most other Canadian studies of voting (see Irvine, 1974), church affiliation now emerges as the strongest factor.

Since the religious and linguistic variables appear to be closely interrelated, a further

Table 2. Unadjusted and Adjusted Indices of Class, Religious, and Linguistic Voting in Four Countries

	Unadjusted Indices			
	Class	Church Affiliation	Church Attendance	Language
Belgium	+10	–	+49	+46
Canada	+ 7	+33	+14	+33
South Africa	+ 9	+27	+23	+66
Switzerland	+21	+49	+46	+25
	Adjusted Indices			
	Class	Church Affiliation	Church Attendance	Language
Belgium	+ 9	–	+39	+37
Canada	+ 6	+23	+ 5	+15
South Africa	+ 4	−10	+ 3	+67
Switzerland	+21	+37	+27	+27

Source: Data from the European Communities surveys conducted in Belgium in 1970 and 1973, the Canadian national election study of 1974, surveys conducted by Market Research Africa in 1974, and the Swiss national election study of 1972.

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[7] On the basis of the same data, Henry H. Kerr (1974, p. 7) finds class, religious (church attendance), and linguistic indices with almost identical values: +22, +21, and +23, respectively. The explanation of this discrepancy is partly that Kerr includes a few minor parties in his analysis in addition to the more important ones considered here, but mainly that he uses the dichotomy of Socialists and Communists versus the other parties for all of his indices; this left-right dichotomy maximizes the index of class voting but is far from optimal for attaining high values for the indices of religious and linguistic voting. See also Sidjanski et al., 1975, pp. 45–61.

examination of the patterns of their influence on party choice is needed. The first step is to take a closer look at the religious factors. Table 3 presents the percentages of support for the "religious" parties among the respondents classified by four frequencies of church attendance and by church affiliation.[8] Switzerland and Catholic Belgium, the two countries with the highest indices of religious voting (church attendance), show very strong monotonic relationships between the frequency of churchgoing and party choice. The differences are smaller and also somewhat uneven in the other two countries. The further classification into Catholics and Protestants in Table 3 shows that frequency of church attendance is an especially important factor only for either the Catholics or the Protestants in each country: for the Catholics in Switzerland and, albeit less strongly, in Canada, and for the Protestants in South Africa. For the other religious groups in these three countries, it is of negligible importance.

Table 3 also shows where the main lines of cleavage appear when church affiliation and church attendance are considered together. In Canada, there are differences in party support *within* both the Protestant and the Catholic communities, but the far greater difference is that *between* Protestants and Catholics. In South Africa, the party choice of non-churchgoing Protestants is very similar to that of the Catholics, and the most obvious cleavage here is between churchgoing Protestants and all others. A similar pattern emerges in the Swiss case: nonpracticing Catholics behave very much like Protestants and unlike their more faithful coreligionists.

These distinctions can be used to compare the relative impact of religion and language on party choice. This is done in Table 4 which presents the percentages of support for the "religious" parties cross-tabulated according to religion, as classified in the previous paragraph, and language. The "religious" parties are also the parties representing linguistic majorities or minorities except in Switzerland where the two dichotomies do not coincide; for this reason, two matrices are given for the Swiss case.

Religion and language are mutually reinforcing determinants of party choice in Belgium, Canada, and Switzerland. In terms of their relative strength, religion has a slight edge over language in Belgium: the indices of religious voting within the Dutch and French linguistic communities are +38 and +44, respectively, whereas the indices of linguistic voting within the frequent church-attending group and the less frequently and nonpracticing group are a somewhat lower +32 and +38, respectively. The Canadian percentages are more difficult to interpret because there are hardly any French-

Table 3. Percentage Support for the "Religious" Parties Among Respondents with ***Different Frequencies of Church Attendance in Four Counrties***

		Percent Supporting the "Religious" Parties in:			
Church Affiliation	**How Often Attending**	**Belgium**	**Canada**	**South Africa**	**Switzerland**
Catholic and Protestant	Very frequently	–	72	73	72
	Frequently	–	61	76	42
	Infrequently	–	56	62	15
	Never	–	57	38	9
Catholic	Very frequently	95	84	41*	86
	Frequently	75	82		63
	Infrequently	43	74	36*	36
	Never	16	74		10
Protestant	Very frequently	–	42	77	5
	Frequently	–	52	78	13
	Infrequently	–	45	63	8
	Never	–	49	40	9

Source: Data from the European Communities surveys conducted in Belgium in 1970 and 1973, the Canadian national election study of 1974, surveys conducted by Market Research Africa in 1974, and the Swiss national election study of 1972.

*These categories had to be combined, because there were not enough cases to present reliable percentages for each of the four categories separately.

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[8] The category of very frequent churchgoers includes those attending more than once a week (Belgium) or at least once a week (the other three countries).

speaking Protestants in the country. Nevertheless, religion emerges as the more important variable because there is relatively less difference between francophone and anglophone Catholics than between Catholic and Protestant anglophones. In Switzerland, religion is clearly the more powerful predictor in Table 4d, but this is not surprising since the parties are here dichotomized in such a way as to accentuate the religious differences. It is therefore of decisive importance that in Table 4e, where the party system dichotomization has the opposite bias in favor of linguistic differences, religion remains the stronger determinant: the two indices of religious voting within the linguistic groups are +36 and +43 compared with the lower indices of linguistic voting of +23 and +30 within the two religious categories.

In South Africa, language and religion do not reinforce each other. In the Afrikaner community, there is a slightly weaker tendency to support the two Nationalist parties among Catholics and nonpracticing Protestants than among practicing Protestants, whereas in the anglophone group the pattern is reversed. The more important conclusion from Table 4c, however, is that language is the overwhelmingly powerful determinant of party support among white South Africans.

The overall pattern of the relative strength of religion and language in the four countries in Table 4 reinforces the earlier conclusions based on the adjusted voting indices of Table 2. Religion is the better predictor in two countries (Canada and Switzerland); language is stronger in only one country (South Africa); and the two factors have approximately the same strength in one of the countries (Belgium). In the group of four countries, religion therefore emerges as the more important determinant.

Tree Analyses of the Patterns of Party Choice

Further evidence, which slightly modifies but generally supports the above conclusion, is provided by the tree analyses in Figures 1 to 4. The party choice dichotomy used in all four country trees is the division between "religious" and "secular" parties, which, it should be remembered, coincides with the division between the linguistic majority and minority parties in all countries except Switzerland. This dichotomy produces the highest total proportion of the variance that is explained by the independent variables. In order to make the results comparable with the analysis of the indices of voting, we have dichotomized the

Table 4. Percentage Support for the "Religious" Parties Among Religious and Linguistic Groups in Four Countries (and Support for the "German Majority" Parties in Switzerland)

a. Belgium	**Dutch**	**French**
(Very) frequent churchgoers	84%	52%
Infrequent and non-churchgoers	46	8
b. Canada	**French**	**English**
Catholics	85%	70%
Protestants	*	46
c. South Africa	**Afrikaans**	**English**
Practicing Protestants	94%	24%
Catholics and non-practicing Protestants	91	31
d. Switzerland (support for the "religious" parties)	**German**	**French**
Practicing Catholics	64%	52%
Protestants and non-practicing Catholics	9	8
e. Switzerland (support for the "German majority" parties)	**German**	**French**
Practicing Catholics	76%	53%
Protestants and non-practicing Catholics	40	10

Source: Data from the European Communities surveys conducted in Belgium in 1970 and 1973, the Canadian national election study of 1974, surveys conducted by Market Research Africa in 1974, and the Swiss national election study of 1972.

*Insufficient number of cases.

independent variables in exactly the same way. The process of splitting the samples into subgroups was continued until no predictor could be found that reduced the variance by at least another 0.3 percent.[9]

The results of the tree analyses are summarized in Table 5, which presents the proportion of the variance in party choice explained by each of the independent variables separately and by all of them together. The table also gives the comparable proportions of the variance explained by the stepwise multiple regression method which, unlike tree analysis, assumes additivity and cannot detect interaction effects. It should be pointed out that for the purpose of this table the tree analysis was extended to the 0.1 percent level, since the variance explained by regression is also measured in tenths of a percent. The two sets of percentages turn out to be remarkably similar. The logic of the two methods necessarily leads to identical percentages of the variance explained by the strongest variable. As far as the other variables and the total reduction of variance are concerned, the tree analysis yields consistently but only slightly higher percentages. This means that interaction effects play a rather small role.

Before we turn to an inspection of the individual country trees, we can state a number of general conclusions. First, it is worth noting that three of the four tree analyses explain a high proportion of the variance in party choice. In Western countries generally the total variance explained is about 25 percent on the average (Lancelot, 1975, pp. 419–20), and, by comparison, the Belgian, Swiss, and South African percentages are very high indeed. This result is especially remarkable because the present analysis considers only four independent variables (and only three in the Belgian case) in contrast to the dozens of social and demographic variables that are usually applied in tree analyses of voting. The exception is Canada where a below-average total of only about 13 percent of the variance can be explained, reflecting "the lack of social definition in the Canadian party system" (Irvine, 1976, p. 355).

Second, the class factor once again turns out to be extremely weak. Tree analyses were also carried out with the parties dichotomized so as to highlight class differences. These trees are not presented here for reasons of space, but the results can be briefly summarized. In the three countries in which the dichotomies favorable to class differ from the dichotomies used in Table 5—Belgium, Canada, and Switzerland—the total proportion of the variance explained is now reduced to 19.5, 3.9, and 17.3 percent, respectively, and the percentage explained by class is

Table 5. Percentages of the Explained Variance in Party Choice ("Religious" vs. "Secular" Parties) in Four Countries

	Tree Analysis*				
	Class	Church Affiliation	Church Attendance	Language	Total
Belgium	1.6%	–	23.6%	10.8%	35.9%
Canada	0.3	11.3%	0.4	1.1	13.1
South Africa	0.5	0.6	0.3	46.6	48.0
Switzerland	1.0	28.0	9.8	0.3	38.9
	Stepwise Multiple Regression				
	Class	Church Affiliation	Church Attendance	Language	Total
Belgium	0.6%	–	23.6%	10.7%	34.9%
Canada	0.0	11.3%	0.2	1.1	12.7
South Africa	0.1	0.4	0.1	46.6	47.2
Switzerland	0.0	28.0	6.3	0.3	34.6

Source: Data from the European Communities surveys conducted in Belgium in 1970 and 1973, the Canadian national election study of 1974, surveys conducted by Market Research Africa in 1974, and the Swiss national election study of 1972.

*The percentages tend to be slightly higher than in Figures 1–4 because the analysis was continued to the 0.1 percent level of explained variance (instead of the 0.3 percent level used in the figures).

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[9] This cutting-off point is lower than the 0.6 percent threshold recommended by the designers of the AID method (Sonquist, Baker, and Morgan, 1971, p. 10), but the lower level has been used in several voting studies (see Rose, 1974a, pp. 214, 261, 309, 644; Liepelt, 1971, pp. 188–89).

only 1.2, 1.1, and 5.2, respectively. The religious variables remain the strongest determinants of party choice in all three cases.

Third, religion now emerges more clearly as the strongest factor. Church affiliation and/or church attendance explain a much higher proportion of the variance than language not only in Canada and Switzerland, but also in Belgium. This conclusion is not changed by the use of the party choice dichotomy more favorable to language in the Swiss case; the total proportion of variance explained is 20.8 percent, in which church affiliation has the highest share of 11.2 percent and language only a modest 4.5 percent. In South Africa, language remains the overwhelmingly powerful predictor.

Fourth, all of the country trees show relatively simple patterns. With four independent variables (three in the Belgian case) it would have been theoretically possible to find as many as sixteen (or, for Belgium, eight) end groups. In fact, the trees have only from four to six end groups. This means that the high total proportions of explained variance are not the result of many small cumulative additions but of large shares of variance explained especially on the first split.

The Belgian tree in Figure 1 is, unlike the other three trees, almost symmetrical. Neither the order of the successive splits nor the nature of these splits contains any surprises: church attendance accounts for the first split, followed by language and class at the second and third levels of splitting, and the frequent churchgoers, Dutch speakers, and non-manual workers are consistently the people who prefer the "religious" to the "secular" parties.[10] The Canadian, South African, and Swiss trees are all

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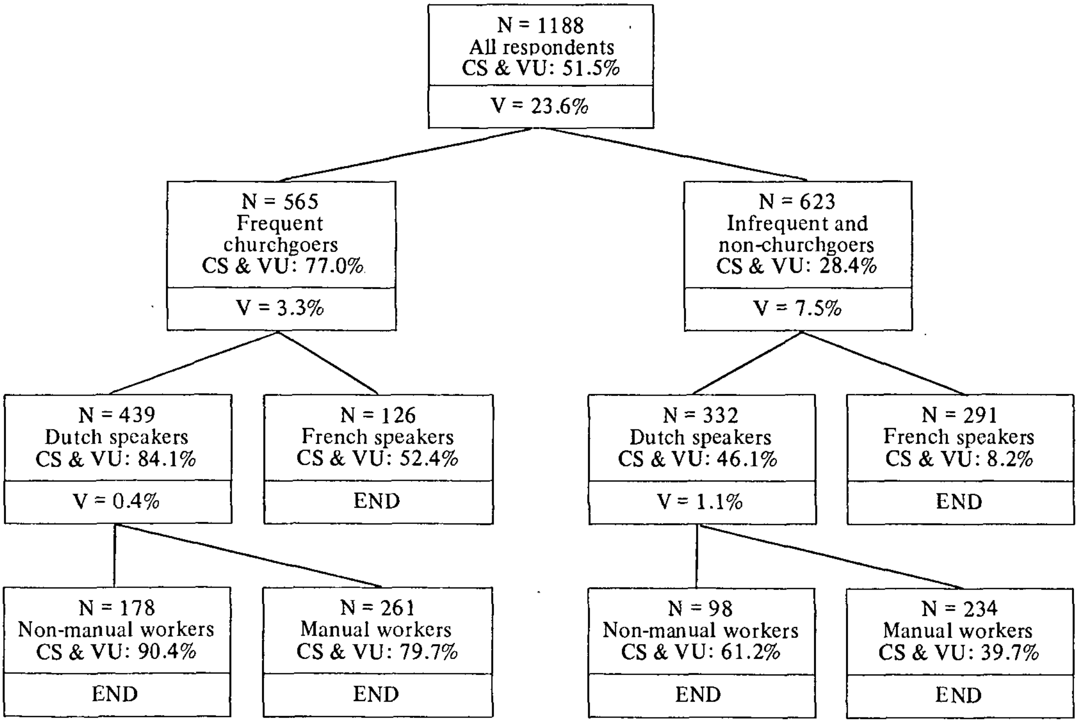
[10] It is surprising that in the tree presented by Keith Hill (1974, p. 103) language does not emerge as a differentiating factor. The most likely explanation is that Hill dichotomizes party choice as left versus right, and that he includes region, which to a considerable extent coincides with language, among his variables.


N = 1188
All respondents
CS & VU: 51.5%
V = 23.6%
N = 565
Frequent churchgoers
CS & VU: 77.0%
V = 3.3%
N = 623
Infrequent and non-churchgoers
CS & VU: 28.4%
V = 7.5%
N = 439
Dutch speakers
CS & VU: 84.1%
V = 0.4%
N = 126
French speakers
CS & VU: 52.4%
END
N = 332
Dutch speakers
CS & VU: 46.1%
V = 1.1%
N = 291
French speakers
CS & VU: 8.2%
END
N = 178
Non-manual workers
CS & VU: 90.4%
END
N = 261
Manual workers
CS & VU: 79.7%
END
N = 98
Non-manual workers
CS & VU: 61.2%
END
N = 234
Manual workers
CS & VU: 39.7%
END

Source: Data from the European Communities surveys conducted in Belgium in 1970 and 1973.

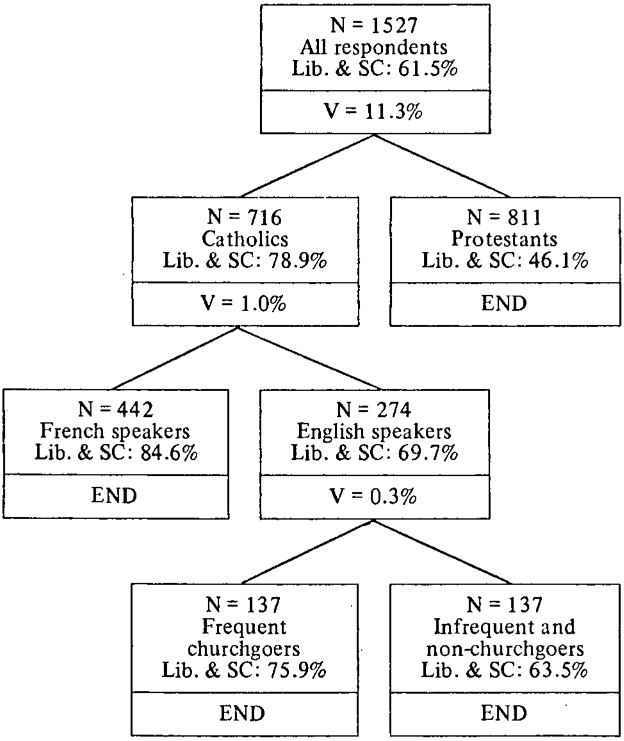
Note: Total variance reduced by 35.8 percent.

Figure 1. A Tree Analysis of Party Choice in Belgium (Christian Social Party and Volksunie)

asymmetrical. In the last two of these cases, this is caused by the fact that the initial split yields a group that cannot be split further because it is highly homogeneous with regard to party choice and contains very little variance: the Swiss Protestants who overwhelmingly reject the Catholic parties, and the Afrikaners who overwhelmingly support the two Nationalist parties. Compared with the Afrikaners' political unanimity, the English speakers can be characterized as "political refracted" (Charton, 1975, p. 118; see also Lever, 1972, pp. 24–26; van der Merwe and Buitendag, 1973, p. 198). The most striking aspect of the South African tree is the great strength of the linguistic factor: close to half of the variance is explained on the first split. The asymmetry of the Canadian tree is not owing to the virtual absence of variance in party choice among the Protestants. In fact, the Protestant group contains much more variance than the Catholics; however, it is linguistically homogeneous, and neither of the remaining variables emerges as a significant differentiator, whereas the Catholic group can be split further—albeit without contributing impressively to the amount of explained variance. The general pattern of the Canadian tree is similar to that of a series of trees based on surveys between 1957 and 1968 presented by Laponce (1972, p. 277): religion consistently emerges on the first split, and region—which, of course, closely corresponds to language—on the second split.

Conclusion: Comparative Theory and the Paradox of Religious Voting

This four-country comparison was described earlier as a "crucial experiment" in Naroll's sense of the term. It may also be likened to a decisive trial of strength, in which religion turns out to be victorious, language is a strong runner-up, and class finishes as a distant third.


N = 1527
All respondents
Lib. & SC: 61.5%
V = 11.3%
N = 716
Catholics
Lib. & SC: 78.9%
V = 1.0%
N = 811
Protestants
Lib. & SC: 46.1%
END
N = 442
French speakers
Lib. & SC: 84.6%
END
N = 274
English speakers
Lib. & SC: 69.7%
V = 0.3%
N = 137
Frequent churchgoers
Lib. & SC: 75.9%
END
N = 137
Infrequent and non-churchgoers
Lib. & SC: 63.5%
END

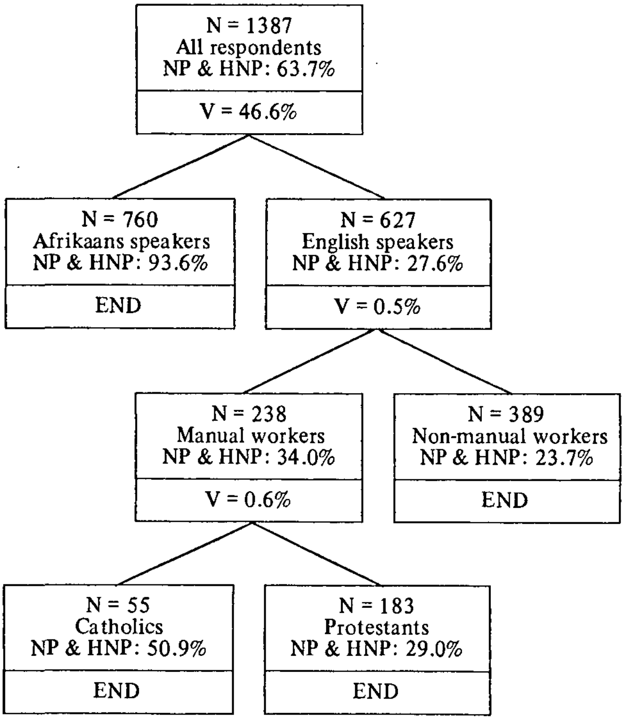
Source: Data from the Canadian election study of 1974.
Note: Total variance reduced by 12.6 percent.

Figure 2. A Tree Analysis of Party Choice in Canada (Liberal and Social Credit Parties)

Can these findings be explained and interpreted in a broader theoretical framework?

The conspicuous weakness of class voting compared with religious and linguistic voting is not really surprising. True, even without a Marxist bias, one would not be unreasonable to assume that elections in Western industrialized societies are, or should be, "the expression of the democratic class struggle" (Lipset, 1960, p. 220), and to regard religious and linguistic attachments as pre-modern forces. However, such "primordial" communal loyalties can be extremely tenacious even in the modern world, and they constitute a formidable obstacle to the development of competing cleavages based on objective socioeconomic interests (see Geertz, 1963, pp. 109–13). As was pointed out above, it is significant that wherever social class and religion are alternative bases of party choice, particularly in the core continental European countries, class voting tends to be weak. And even in the Anglo-American democracies, it can be argued–on the basis of Alford's own evidence on class voting in these countries–that "class is the major determinant of voting behavior *only if* no other cleavage happens to be present" (Sartori, 1969, p. 76). Social class is clearly no more than a secondary and subsidiary influence on party choice, and it can become a factor of importance only in the absence of potent rivals such as religion and language.

Neither is it surprising that linguistic voting turns out to be relatively strong in the four linguistically divided countries. A shared language is regarded as one of the principal building blocks of nationalism both in the traditional literature on this subject and in Karl W. Deutsch's (1953) more refined concept of a nation as an intensive network of communication. Because language is a crucial differentiator among nations, it is bound to be a major cleavage and a strong source of partisan differences in "nations" that are not linguistically homogeneous. The finding that language is a strong determinant of party choice also accords

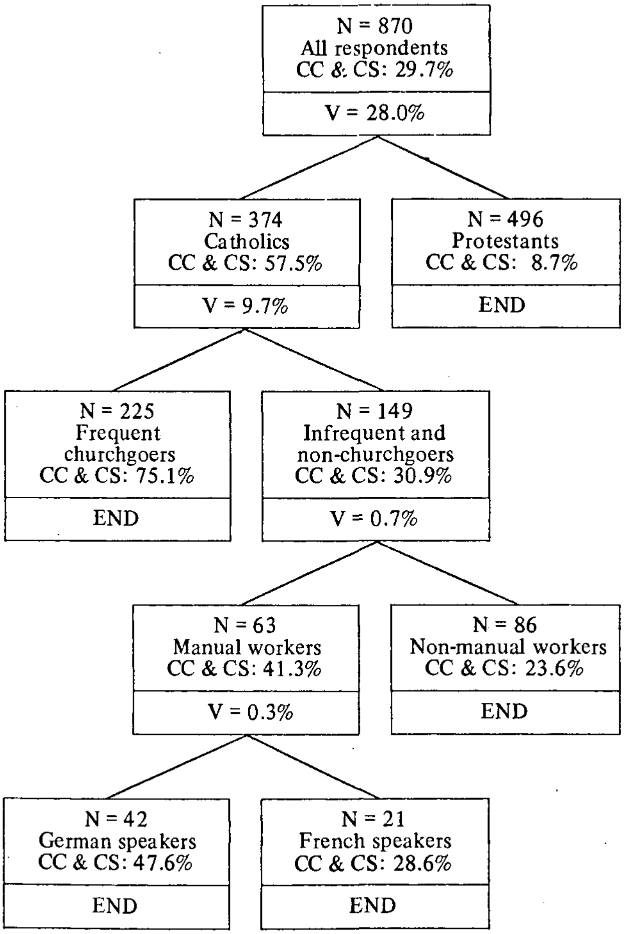


Source: Data from surveys conducted by Market Research Africa in 1974.
Note: Total variance reduced by 47.8 percent.

Figure 3. A Tree Analysis of Party Choice in South Africa (National and Reconstituted National Parties)

with the developmental theory of cleavage structures and party systems expounded by Seymour M. Lipset and Stein Rokkan (1967): cultural-territorial conflicts are one of the four basic sources of party system cleavages that they distinguish.

The more surprising—and the theoretically more interesting and challenging—empirical finding concerns the relative strength of religious and linguistic voting: religion emerges as the more important dimension of party choice in the 1970s, whereas especially in Belgium and Canada the linguistic-ethnic cleavage has been the more salient dimension of political conflict in recent years. For instance, Meisel (1974, p. 9) considers religion to be "of virtually no political importance in contemporary federal politics" in Canada, whereas, of course, the conflict between francophone and anglophone Canada is of the greatest political significance. Similarly, linguistic issues have in recent years been much more prominent than religious problems in Belgium. Conversely, language has remained the paramount determinant of party choice in South Africa, although, as Hendrik van der Merwe and J. J. Buitendag (1973, p. 190) observe, "there is much evidence that the traditional divisions between the two language groups are tending to become muted." Switzerland is a more doubtful case. In recent years,


N = 870
All respondents
CC & CS: 29.7%
V = 28.0%
N = 374
Catholics
CC & CS: 57.5%
V = 9.7%
N = 496
Protestants
CC & CS: 8.7%
END
N = 225
Frequent churchgoers
CC & CS: 75.1%
END
N = 149
Infrequent and non-churchgoers
CC & CS: 30.9%
V = 0.7%
N = 63
Manual workers
CC & CS: 41.3%
V = 0.3%
N = 86
Non-manual workers
CC & CS: 23.6%
END
N = 42
German speakers
CC & CS: 47.6%
END
N = 21
French speakers
CC & CS: 28.6%
END

Source: Data from the Swiss national election study of 1972.

Note: Total variance reduced by 38.6 percent.

Figure 4. A Tree Analysis of Party Choice in Switzerland (Christian Conservative and Christian Social Parties)

the Jura problem has called attention to the fact that even Swiss politics is not immune to linguistic tensions, but the Jura question is primarily a regional rather than a national issue and it is also just as much a religious as a linguistic problem. At the national level, neither religious nor linguistic issues can be said to have high salience.

Two explanations of the paradox may be offered. One follows the well-known argument of Lipset and Rokkan (1967, pp. 50–51) that party systems tend to reflect the political cleavages of the periods in which the parties came into existence, instead of the dimensions of contemporary political conflict. The conflicts of the past have structured the political parties and the differences between them, and are able to survive as entrenched atavisms in the party systems. The party systems in turn structure the electoral contests, even when other political dimensions have become more salient. Following this line of reasoning, Mildred Schwartz (1974, p. 589) argues that in Canada "class-based voting exists; it is consistent class-based parties that are missing." Similarly, one can say that in Belgium, Canada, and Switzerland, the potential strength of linguistic voting is suppressed by the religiously oriented structure of the party systems. The Belgian case also demonstrates, however, that the "freezing" of the party system in the 1920s was not absolute: Flemish linguistic parties already made their appearance in 1919 and have survived in the form of the Volksunie, but the Walloon and Francophone parties originated in the 1960s in a long and solidly established party system. Of course, the presence of these specifically linguistic parties also accounts for the considerable strength of linguistic voting in Belgium, which does not lag very far behind the strength of religious voting.

The second explanation of the paradox of strong religious voting is based on the notion that political parties are important, but not the only, vehicles for the articulation and representation of various kinds of interests. In Belgium, Canada, and Switzerland, a significant difference between the religious and linguistic communities is that the former tend to be geographically dispersed whereas the latter are regionally more concentrated. Consequently, the most suitable organs for the representation of religious interests have been the political parties, whereas the strongest organs for the representation of the linguistic interests could be the subnational governments, especially if they possessed or acquired a high degree of political autonomy: the provincial and cantonal governments in federal Canada and Switzerland and the recently established cultural councils in semi-federal Belgium (Dunn, 1972; Lijphart, 1977, pp. 90–97, 121–24). The deviant case of South Africa with its extremely strong linguistic voting patterns significantly reinforces this argument: because South Africa has a unitary form of government and because its linguistic communities are geographically interspersed to a high extent, the linguistic interests have had to protect themselves mainly through the party system.

Hence the greater strength of religious compared with linguistic voting in Belgium, Canada, and Switzerland is neither the result of the greater contemporary salience of religious issues in these countries (with the partial exception of Switzerland), nor exclusively the outcome of an atavistic survival of past religious conflicts. At least to some extent, it is also the effect of the presence of alternative, regional-federal, structures for the articulation and representation of the linguistic interests.

Appendix: Survey Data and Operational Definitions

(a) *The data.* The surveys were conducted at approximately the same time: the Belgian surveys in 1970 and 1973, the Swiss survey in 1972, and the Canadian and South African surveys in 1974. The Canadian and the Swiss surveys were election studies, although the Swiss survey was not held until several months after the elections. The South African data were collected in an election year but not in connection with explicit electoral research. The only major Belgian election study done so far, in 1968, could not be used because it did not include any question about the respondents' language (Delruelle, Evalenko, and Fraeys, 1970); instead, two more recent sociopolitical, but non-election, surveys had to be used. It would have been desirable to compare the major social determinants of party choice in the four countries not only in the 1970s but also their trends over extended periods of time. Unfortunately, with the exception of Canada, data of sufficient quality for such a diachronic analysis of voting in these countries are not available.

The multivariate nature of the analysis and the choice of methods required that the sizes of the national samples be relatively large. This is the reason why, in the Belgian case, two similar surveys conducted in 1970 and 1973 were combined. However, no additional data were available to increase the size of the South African sample. In fact, the latter already consists of two separate subsamples of about equal size (a sample of Afrikaners and one of English-speaking white South Africans to whom similar questionnaires were administered) which could be combined by applying appropriate weighting procedures. The South African data were collected in cities, towns, and villages, and exclude the rural, mainly farming, population, but this does not affect their comparability with the data from the other three countries because the manual versus non-manual classification that was used required the exclusion of farmers from these samples, too. All four samples can therefore be regarded as representative samples of the national voting populations. The exact numbers of respondents in the samples are as follows: Belgium (1970), N = 1296; Belgium (1973), N = 1266; Canada (1974), N = 2562, weighted N = 2445; South Africa (1974), N = 641 (Afrikaners) + 659 (English speakers) = 1300, weighted N = 2175; Switzerland (1972), N = 1917.

(b) *Operational definitions of the variables.* Social class was operationally defined in terms of the occupation of the head of the household to which the respondent belonged. In order to assign these occupations to the manual and non-manual strata, we used the criteria listed by Alford (1963, pp. 70–71). Farmers are the only significant group that cannot be accommodated in this dichotomous classification.

The two religious variables that were considered are church affiliation and church attendance. The former was dichotomized into Protestant and Catholic categories with all respondents of other faiths and without religious affiliation being left out of consideration. This is the most meaningful and obvious classification for the Canadian and Swiss cases. It may be argued that the more significant dividing line in South Africa lies between the adherents of the Dutch Reformed churches on the one hand and Anglicans, Methodists, Catholics, and Jews on the other hand. Such a dichotomy coincides so closely with the linguistic-ethnic difference between Afrikaners and English speakers, however, that it turns church affiliation into a virtual replica of the linguistic variable (see Peele and Morse, 1974, p. 1526). For the sake of comparative consistency, too, it makes more sense to use the Catholic-Protestant dichotomy in all three countries.

The dichotomization of the church attendance variable presented slightly more serious problems because the categories in the various surveys were not always the same. To keep the dichotomy as simple and straightforward as possible, we drew the dividing line between frequent churchgoers (attending at least twice a month) and those attending church only infrequently or never.

The linguistic variable was operationally defined as the language usually spoken at home or the mother tongue. A question about the home language was asked in the Belgian, Canadian, and South African surveys, but the Swiss survey only inquired into the mother tongue of the respondents. It is a reasonable assumption, however, that the mother tongue and the usual language spoken at home coincide in the vast majority of cases.

The respondents' choice of political party may be elicited by means of several different questions. The principal possibilities are: the respondents' last vote, their intended vote (if an election is to take place in the near future), their probable vote (in answer to the hypothetical question about how they would vote if an election were held at the time of the survey), and their general party preference or party identification. In American voting behavior research a clear conceptual distinction is usually drawn between these variants, especially between party identification and actual voting choice. In other countries, there tends to be such a close relationship between the two that they are virtually indistinguishable. As Budge and Farlie (1976, p. 123) point out, party identification in non-American political contexts predicts actual voting so well that one must "suspect its empirical independence of voting choice, its conceptual antecedence and its explanatory capacity." Because the data of the present study were collected partly in election surveys and partly in non-election surveys, the best method to maximize comparability on this dimension was to avoid the intended or most recent votes as indicators of party choice and to use party preference (Canada and Switzerland), the probable vote (South Africa), or a combination of the two (Belgium).

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The Roots of Interprovincial Inequality in Education and Health Services in China*

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The key hypotheses which find support in this study of the determinants of variations among the provinces of China in the provision of health and education services are: (1) that interprovincial variations in the provision of educational and health services can be explained largely by economic and ecological factors; (2) that specific levels of education and various types of medical services are responsive in different ways to changes in particular variables. For instance, the survival of cooperative health programs in the 1970s is much more sensitive to alterations in agricultural production than is the urban hospital system, which is much more dependent upon urban economic growth. This refines Dye's findings and makes them more applicable to Third World systems. (3) Central policy choices are therefore important because choices relating to investment strategy and program structure ultimately determine the relationship between the economy and the programs.

The People's Republic of China has made important, and comparatively rapid, progress in reducing inequalities in real income (Lardy, 1975b) and making primary education and basic medical services more universally available (Lampton, 1978). Between 1949 and 1976, generally, the regime pursued policies of keeping urban wages relatively constant while boosting real rural incomes. In 1949, cities like Peking had between 10 and 20 percent of their populations in primary school while outlying provinces had less than 1 percent; by the mid-1970s, Chinese authorities claimed to have practically universalized primary education. Since 1968, equally vigorous attempts have been made to equalize access to basic health services by building a system based on paramedical workers.

These serious attempts at achieving equality of access to specific categories of educational and health services make the inequalities among the provinces so interesting from both a practical and a theoretical viewpoint. This study will ask: How have five major Chinese provinces performed comparatively in providing access to primary and secondary education, hospital beds, and rural medical services since 1949? What factors best account for the observed variations, after we have considered many possible variables? Are economic factors decisive in determining program performance, as most of the literature pertaining to industrialized societies suggests, or does leadership choice need to be considered as well? If both play a role, what is the relationship between choice and economic determinism? Also, when economic variables are considered, are different educational and health programs sensitive to different parts of the economy? When one looks at leadership choice, which leaders, at what levels, are most important with respect to which policy areas?

The major findings of this study are twofold: (1) Economic variables are the principal determinants of the pattern of interprovincial performance for *any given* program, but, leadership choices relating to program financing, investment strategies, and program priorities will demonstrably affect the way in which the economy interacts with the program. For instance, if Chinese leaders in Peking decide to fund primary education out of local taxes, the interprovincial distribution may well be different from providing such schooling out of revenues coming from the national treasury. Similarly, if Chinese leaders decide to concentrate industrial and urban growth in a few areas, the pattern of distribution of hospital beds and middle schools will be different from that of a situation in which urban and industrial development occur more evenly.

As Huntington and Nelson (1976, p. 20) have persuasively argued, the kind of economic determinism which asserts "that the causal flow would be from economics to politics rather

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than in the reverse direction" is overly simplistic. While the studies of Dye (1966) and Wilensky (1975) are important in relating economic factors to program performance, the study of interprovincial variation in China gives substance to Huntington's assertion that leadership choice decisively influences economic variables. Also, leadership choices affect *which* economic variables are most salient to *which* program.

(2) This study clearly suggests that the critical political choices are those concerning investment priorities, funding strategies, and program priorities. These decisions are invariably made in Peking, leaving provincial leaders with responsibility for implementing and refining policy. Confirming Falkenheim's (1972) findings for Fukien Province, this study suggests that interprovincial variations in performance do *not* represent the accumulated result of independent decisions made at the provincial level. As Dye (1966) found in his analysis of the United States, we have found that the political characteristics of subnational units in China have not been the principal determinants of provincial performance.

Methodology and Rationale

Were the analyst, initially, to attempt to formulate a tentative "answer" to these questions derived from a simultaneous examination of all 29 provincial-level units in China, the costs and difficulties of collecting data for all possibly important variables would be staggering. Consequently, I have adopted an alternative strategy. Five important and variably performing provinces (accounting for about 28 percent of China's population) have been intensively examined: Anhwei, Hunan, Kwangtung, Shansi, and Shantung. This analysis identifies variables which best account for the observed interprovincial variations in performance. Having formulated clear hypotheses from the case provinces, we will be able (in the future) to collect appropriate data for the remaining two dozen provincial-level units in order to test the degree to which the observed relationships hold nationally.

Two criteria were employed in selecting the case provinces: (1) Relatively complete and reliable performance data had to be available. The indicators of performance to be used were: the percent of provincial population separately enrolled in regular primary and general middle schools, the number of hospital beds per one thousand population, and the percentage of a province's production brigades (*sheng-ch'an tatui*) with "cooperative health programs." For a thorough discussion concerning the choice and reliability of these indicators, see Lampton (1978). From those provinces for which suitable performance data were available, we selected provinces which provided a wide variety of educational and health service levels. (2) The provinces had to be geographically dispersed in order to assess the possible importance of various pre-liberation experiences, North-South differences, economic disparities, and region (Skinner, 1977). Defense areas and sparsely populated minority regions were excluded from consideration because their military and political sensitivity has given them a salience to the national leadership that has altered their performance patterns (Teiwes, 1971). Also, immigration to these areas has made demographic data for these areas particularly unreliable.

The following analysis is organized by program, dealing sequentially with factors affecting the availability of primary education, secondary education, hospital beds, and cooperative health programs in the five provinces. This format focuses attention upon providing comparatively derived explanations for performance variations.

What about data reliability and the implicit biases of the measures employed? Looking at Chinese statistical data generally, we find that the reliability and availability of such information has varied over time. During the late 1950s, the statistical system broke down as a consequence of the Great Leap Forward. In the early 1960s, provincial budgets, so freely available during the 1950s, were no longer disseminated in ways that made them accessible to outside analysts. Finally, during the Cultural Revolution of 1966–1969, the statistical system once again was in disarray. Consequently, the most reliable statistical data are available for the 1950s and the 1970s. I obtained provincial-level data on hospital beds, school enrollments, insurance coverage, budgetary expenditures, and gross value of industrial output for the 1950s from provincial newspapers which printed government work reports and budgets. These data do not generally contain large internal inconsistencies or unexplainable variations. Indeed, these figures document problems to which politicians and administrators were referring simultaneously.

For the 1970s, I have culled statistics from the national press and provincial radio broadcasts. Recent figures are comparable with the pre-1958 series. For instance, Emerson, Lardy, and Field have found that figures on Chinese gross value of industrial output (used below) are of sufficient reliability and consistency to be very useful in economic analyses. Tom Wiens (1975) has found provincial agricultural performance data equally useful. While the reliability of Chinese data is accepted with the noted qualifications, a caveat is appropriate. One should not draw profound conclusions from small isolated variations.

As to the problem of biases implicit in the measures used, several observations are required: First, all of the measures employed in this study have been used by the Chinese themselves. It is true, however, that we have not employed all the performance measures that the Chinese have used at any given time. This is the case because some of the indicators appear to be more reliable, meaningful, and comparable across provinces than others. However, the fact that we have used some measures to the exclusion of others is potentially important. For instance, the use of hospital beds as an indicator of health service availability distorts the assessment in favor of urban (curative) health care when Chinese priorities *may* stress rural (preventive) services. Alternative measures such as "health personnel" might have alleviated this particular problem, but, these data were not regularly released on a provincial basis. As well, "health personnel" variably includes nurses, doctors, herbalists, acupuncturists, sanitary workers, and paramedics; one cannot be confident that the term has much uniformity of meaning across either time or space. Finally, the fact that the Chinese kept regular statistics on hospital beds during the 1950s tells us something about their priorities at that time. When they became more interested in rural medical care in the late 1960s and 1970s, they began to release regularly appropriate performance measures which we have used for that period.

Primary Education in the 1950s and Mid–1970s

Making primary education nearly universally available in the five provinces during the mid-1970s has been a tremendous achievement (see Table 1). Table 2 shows that inequality in provincial rates of primary school enrollment was evident during the 1949–1957 period.[1] During that time, Shansi Province performed best in providing primary enrollments, Anhwei did least well in this respect, and the other three provinces were arrayed between these two.[2] What factors appear more nearly to account for these divergences in the 1949–1957 period and what has produced the more uniform enrollment rates of the mid-1970s?

As developed below, per capita gross value of industrial output (hereafter per capita GVIO) and percent of provincial population in urban areas (with which per capita GVIO is intimately associated) are most closely linked to the observed interprovincial differences in primary enrollment levels during the 1949–1957 period. These relationships appear to have existed because primary education

Table 1. Percentage of School-Age Children in Regular Primary Schools, 1965 and 1972–1976 (by Province)

	1965	1972	1973	1974	1975	1976
Anhwei			88.9*	92.2		96.0
Hunan			98.9		95.0	
Kwangtung	83.5		92.0			
Shansi						
Shantung				94.3		

Source: All enrollment data from which these statistics were generated are from Chinese provincial radio broadcasts as monitored by the *Foreign Broadcast Information Service: Daily Report, People's Republic of China* (1973–1976), and *Summary of World Broadcasts* (1973–1974). Individual citations not included for space reasons will be provided upon request.

Note: It would be desirable to make Tables 1 and 2 statistically comparable, but this cannot be done without data on the age structures of each province. Such information is unavailable.

*This figure was for *rural* areas, so the province-wide rate is undoubtedly somewhat higher.

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[1] Had this author chosen such provinces as Tibet, Kweichow, or Tsinghai, the inequalities would have been much greater.

[2] Wu (1967, p. 91) classifies Shansi as relatively less developed than Anhwei. One must carefully define the meaning of "developed." Wu generally uses aggregate indicators rather than per capita measures. Aggregate measures favor provinces with large populations.

required both a local revenue base adequate to finance educational programs and sufficient community stability to assure continuity in primary school attendance. Both of these conditions were more prevalent in urban and industrial settings than in rural areas.

Peking's conscious commitment to equalizing industrial and urban growth during the 1950s (Lardy, 1975a and 1975b) appears to have provided the basis for the more equal distribution of primary enrollments across provinces which was evident by the mid-1970s. As well, central policy makers have sought to change the way primary education is funded so as to reduce some of the financial advantages which accrued to urban areas in the 1950s. In short, while we have found quite predictably that economic and structural variables are decisive in determining program performance, the analysis of these five Chinese provinces strongly suggests that conscious central policy choices can affect these master variables in such a way as to promote or retard the realization of equality among provinces.

Provincial political and social attributes such as leadership stability (see Table 3, column f), percentage of provincial population in the Chinese Communist party (Table 3, column h), and the availability of surface transportation (Table 3, column e) do not appear to be powerful predictors of provincial performance for any of the programs examined in the case provinces. Consequently, while data on these provincial attributes have been supplied in the relevant tables, they generally will not be discussed.

Before we establish that urbanization, average per capita GVIO, and level of primary school enrollment are highly correlated, and before we explain why such relationships exist, a brief qualitative description of Shansi (the most successful province in providing primary school enrollments in the 1949–1957 period) and Anhwei (the province least successful in this respect) will provide an important qualitative context for the rest of the analysis.

In the early and mid-1950s, Anhwei was a province with severe social, political, and economic difficulties; it ranked poorly among most of the comparative dimensions displayed in Table 3. Anhwei was a middling producer of foodgrain per capita (Wiens, 1975, p. 90), had the lowest urbanization rate among the five provinces, and had the lowest average per capita gross value of industrial output (GVIO). These problems reflected and contributed to a particularly knotty set of environmental and administrative problems in Anhwei. Anhwei's leaders desperately sought to control the Huai, Ying, Ko, Yangtse, and P'i Rivers. In 1955–1956, 50 percent of the province was affected by natural disasters. The year before (1954), Anhwei's leadership acknowledged that it was only thanks to timely central aid that "the people of Anhwei basically tided over the famine and achieved the target of saving people from starvation" (Yu, 1955, p. 41). These calamities, the subsequent social dislocation, and the government's ill-advised grain procurement policies in 1955, produced substantial violence (Yu, 1955, pp. 41–42).

In contrast to Anhwei's plight, Shansi did exceptionally well in providing primary education (Table 2). In 1953, Shansi was the most urbanized of the five provinces (but was by no means the most urbanized in China) and it had the largest and most rapidly growing per capita GVIO (see Table 4). The designation of Shansi as an "industrial keypoint" (*chung-tien*) for the First Five-Year Plan (1953–1957) meant that it would receive greater than average funds for "state construction," funds that (as we shall

Table 2. Primary Enrollments in Five Case Provinces, 1949–1958 (Percent of Provincial Population Enrolled in Regular Primary School)

	1949	1950	1951	1952	1953	1954	1955	1956	1957	1958
National Average	4.5	5.2	7.6	8.8	8.7	8.5	8.7	10.1	10.0	13.1
Anhwei							6.5	8.6	8.4	
Hunan				8.2			9.1	10.9	10.2	13.2
Kwangtung	5.7		6.9	8.6	9.4	9.1	8.7	10.9	10.7	13.7
Shansi	7.0			11.2			10.4	12.0	12.1	
Shantung	4.3						8.2	9.3		

Source: All enrollment data from which these statistics were generated are from the following Chinese provincial newspapers: *Anhwei Jih-pao, Hsin Hunan Pao, Nan-fang Jih-pao, Shansi Jih-pao, Tachung Jih-pao, Tsingtao Jih-pao,* and *Wen Hui Pao.* Precise documentation will be provided upon request. These figures were collected by the statistical departments in each province and they were internally consistent.

Table 3. Selected Characteristics of Five Chinese Provinces and Ranking of Each by Attribute

	Grain Output/ Capita (in Catties)[a]	GVIO/Capita 1949 and 57 Average (*yüan*)[b]	Percent of Population Urban, 1953[c]	Capital Investment/ Capita (in *yüan*)[d]	Roads–km/ 1000 km^2, Circa 1950[e]	RR's–km/ 1000 km^2, Circa 1950[e]	Teiwes' Stability Category[f]	HEW Expenditures/ Capita, 1956 (*yüan*)[g]	Percentage of Population in CCP, Mid-1956[h]
Anhwei	521 rank 3	28 rank 5	6.7 rank 5	3.6 rank 2	54.3 rank 3	4.6 rank 3	III	4.9	.83 rank 4
Hunan	570 rank 1	31 rank 4	7.0 rank 3	N.A.	26.8 rank 5	4.3 rank 4	III	3.5	.80 rank 5
Kwangtung	551 rank 2	64 rank 2	12.2 rank 2	3.6 rank 2	58.1 rank 2	2.9 rank 5	II	5.1	.93 rank 3
Shansi	486 rank 4	66 rank 1	12.9 rank 1	6.9 rank 1	53.9 rank 4	7.9 rank 1	I	6.7	2.92 rank 1
Shantung	423 rank 5	47 rank 3	6.9 rank 4	2.0 rank 4	94.7 rank 1	7.3 rank 2	IV	3.6	2.14 rank 2

Source: As indicated in notes below.

[a] This indicator is the average of the yearly per capita foodgrain production of each province for the entire 1949–1957 period. Grain figures from Kang Chao, *Agricultural Production in Communist China, 1949–1965* (Madison: University of Wisconsin Press, 1970), pp. 302–03. These are official Chinese figures and exclude soybeans. The population figures are from John Aird, "Population Estimates for the Provinces of the People's Republic of China: 1953 to 1974," *International Population Reports,* Series P–95, No. 73 (Washington, D.C.: Department of Commerce, 1974), p. 23. All numbers have been rounded to the nearest catty.

[b] This indicator is the average of the per capita gross value of industrial output (GVIO) for the years 1949 and 1957. Yearly provincial per capita GVIO figures are displayed in Table 4. Absolute GVIO figures are from Robert M. Field, Nicholas Lardy, and John Emerson, "A Reconstruction of the Gross Value of Industrial Output by Province in the People's Republic of China: 1949–73," *Foreign Economic Report,* No. 7 (Washington, D.C.: Department of Commerce, 1975), p. 9. The same population data were used for this computation as above.

[c] These percentages represent the proportion of persons living in urban places of 2,000 or more in 1953. These estimates may be found in Morris B. Ullman, "Cities of Mainland China: 1953 and 1958," *International Population Reports,* Series P–95, No. 59 (Washington, D.C.: Department of Commerce, 1961), pp. 11–12.

[d] This figure is the average of annual provincial per capita budgetary allocations for "state economic construction" from 1955 to 1957. The population data are from Aird (1974), p. 23.

[e] Both road and railroad miles were ascertained by using an official Army Map Service 1:250,000 map series with data current through approximately 1950. All road and railroad miles were counted, even though some of the mileage may not have been serviceable at this time. Roads included were: "fair, dry weather, loose surface or dirt" and "all weather." With respect to railroads, both single and double track lines have been included.

[f] These rankings were assigned by Frederick Tiewes in his article, "Provincial Politics in China: Themes and Variations," in John M. H. Lindbeck (ed.), *China: Management of a Revolutionary Society* (Seattle: University of Washington Press, 1971), pp. 152–53. These stability categories refer only to *leadership* continuity as measured by the number of first party secretaries and changes in the composition of various leadership "core groups" in the 1956–1966 period. A low number indicates high stability and a high number low stability.

[g] These per capita "health, education, and welfare" expenditure figures are from Nicholas R. Lardy, "Centralization and Decentralization in China's Fiscal Management," *China Quarterly,* No. 61 (1975): 37.

[h] From Frederick C. Teiwes, "Provincial Politics in China: Themes and Variations," in John M. H. Lindbeck (ed.), *China: Management of a Revolutionary Society* (Seattle: University of Washington Press, 1971), p. 165.

see) helped increase the local tax base, created demand for education, and created a stable social base that would permit children to attend school without frequent interruptions. In the 1955–1957 period, almost twice as much "state construction" investment per capita was pumped into Shansi as any of the other case provinces (Table 3, column d). Finally, while this contribution is impossible to quantify, Shansi's pre-liberation leader, Yen Hsi-shan, had provided vigorous leadership in the development of primary education (Gillin, 1967, p. 302).

Above, we asserted that average per capita GVIO (Table 3, column b) and the percent of provincial population living in urban places (Table 3, column c) were the most potent determinants of a province's performance in providing primary school enrollments in the 1949–1957 period. What evidence is available to support this assertion and what would account for such relationships? Finally, has the greater equality in provincial primary school enrollment rates characteristic of the mid-1970s been accompanied by corresponding changes in these variables?

Looking at the pattern of enrollments in primary education among the provinces during the 1949–1957 period (see Table 2), and comparing that to the provincial attributes displayed in Table 3, we can see that Shansi, the most urbanized province with the highest average per capita GVIO in the 1949–1957 period, consistently had the largest primary school enrollments. The least urbanized province, with the smallest average per capita GVIO, Anhwei, was persistently lowest. Similarly, the second most urbanized province, with the second highest average per capita GVIO in the 1949–1957 period (Kwangtung), was the second best at providing primary school enrollments. Conversely, Shantung, the second to the last province in degree of urbanization (but not average per capita GVIO) was second to last in primary enrollments.

The apparent relationships between average per capita GVIO, percent of provincial population residing in urban places, and a province's level of primary enrollment are also apparent from statistical tests.[3] While statistics generated with so much missing performance data and a small number of provinces is of quite limited significance, such analysis supports the conclusions drawn above and, at a minimum, suggests that future researchers wishing to examine all 29 Chinese provincial-level units would be well advised to include these two variables in their analysis.

What concrete factors account for the apparently close linkage between urbanization, average per capita GVIO, and the level of primary school enrollment? Clearly, variables

Table 4. **Per Capita Gross Value of Industrial Output for Five Chinese Provinces, 1949–1957 (in *yüan*)**

	1949	1952	1953	1954	1955	1956	1957
Anhwei	11.0	20.9			28.5	35.5	45.1
Hunan	11.5	23.5				45.2	49.8
Kwangtung	25.5	50.5	64.5	75.5	78.7	91.7	101.6
Shansi	16.7	46.0					116.1
Shantung	20.5	42.6					74.2

Source: The absolute GVIO figures are from Robert Field, Nicholas Lardy, and John Emerson, "A Reconstruction of the Gross Value of Industrial Output by Province in the People's Republic of China: 1949–73," *Foreign Economic Report,* No. 7 (Washington, D.C.: Department of Commerce, 1975), pp. 20–22. Population data are from John Aird, "Population Estimates for the Provinces of the People's Republic of China: 1953 to 1974," *International Population Reports,* Series P–95, No. 73 (Washington, D.C.: Department of Commerce, 1974), pp. 5, 23.

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[3] We analyzed the relationship between average per capita GVIO (Table 3, column b) and the level of primary enrollment for those years for which we have the *relatively* most complete primary education performance data (1949, 1952, 1955, 1956, and 1957). When the data were analyzed as interval-level data, the strength of positive association, as measured by Pearson's R, varied from .91 (significant to the .12 level) to .64 (significant to the .27 level). When the data were expressed as rank orders for each year, the Tau B's varied from 1.0 to .6. The percent of provincial population in urban areas (Table 3, column c) is positively correlated with the level of primary school enrollment. The strength of association as measured by Tau B varied from 1.0 to .8 and when measured by Pearson's R, varied from .92 (significant to the .12 level) to .67 (significant to the .10 level). Complete statistical data will be supplied upon request.

affecting both the system's capacity to *supply* education and the public's *demand* for primary education are important.

Looking at the supply side of the equation first, we note that the method by which primary education was funded during the 1949–1957 period was critical. Throughout the 1950s, primary education was importantly supported by locally controlled "extra-budgetary" funds (*yu-suan-wai tsu-chin*) generated from the *local* industrial surtax, the local agricultural surtax, the commercial enterprise levy, house taxes, utilities surcharges, wharfage fees, and other minor levies (*Nan-fang Jih-pao*, August 27, 1951). While the specifics of the tax system changed somewhat throughout the 1949–1957 period, these locally controlled extra-budgetary resources remained of genuine importance to primary education (Donnithorne, 1967, pp. 389, 393). While we have no systematic and comparehensive data to demonstrate what percentage of these local extra-budgetary funds came from the industrial surtax and what percentage from other levies (and there were necessarily wide variations between localities), we do know that industrial and commercial taxes were generally two to four times as large a component of *provincial* revenue as the agricultural taxes (Chen, 1967, pp. 451–67). One presumes that local revenues generated from surtaxes on these levies reflected the same predominance of industrial and commercial areas.

Provincial officials in Shansi and Shantung provinces (*Tachung Jih-pao*, August 17, 1957) explicitly noted the importance of these locally derived and locally controlled revenues to primary education. For instance, Shansi's Wu Kuang-t'ang explained,

> Expenditures for the social, cultural, and educational program come to 105,708,000 *yüan*. . . . With the addition of 5,234,000 *yüan* in local supplementary funds to meet the needs of the primary schools, the total figure represents an increase of 44.82 percent over the final budget figures for 1955 (*Shansi Jih-pao*, December 9, 1956).

As a consequence of these financing arrangements, the provision of primary education in the 1950s (prior to 1958) was importantly influenced by the locality's revenue-generating ability; there had to be a revenue base sufficient to support primary education. This aspect of the tax system appears to have favored localities with extensive industrial and commercial development; they would have had the most plentiful local extra-budgetary resources with which to support primary education. In short, the way in which primary education was funded rewarded relatively urbanized provinces like Shansi and Kwangtung and made it more difficult for provinces like Anhwei and Shantung to achieve parity in enrollment levels.

In evaluating the case of Kwangtung, in addition to its comparatively substantial industrial and agricultural base, we must recognize an "idiosyncratic" factor. The province had 6.4 million overseas Chinese; many received financial support from relatives abroad. Chinese abroad were exhorted to use these remittances to support schools and other construction activities. The amounts involved were far from trivial and they had a disproportionate impact on education.

> The CCP [Chinese Communist party] gave its blessing to existing Overseas Chinese schools and revived some which had become defunct, it created new ones and in some cases simply conferred the title "Overseas Chinese" on existing schools, and it persuaded groups of dependents to "request" the establishment of such schools in their own districts. The Party then appealed for donations from Chinese abroad and suggested to their dependents in China that they might donate some of their savings or "surplus" remittances (Fitzgerald, 1972, p. 132).

If we conclude from the Shansi and Kwangtung cases that a solid local revenue-generating ability was essential for the development of primary education, and industrial and commercial development were crucial to providing such resources, our analyses of Shantung and Anhwei reinforce that finding. Anhwei, which had comparatively few children enrolled in primary schools, had the least developed urban sector and had the lowest average per capita GVIO (see Table 3). Shantung had only a slightly more developed urban and industrial sector. This, combined with the fact that neither province was a major recipient of per capita capital construction investment (see Table 3, column d), meant that both provinces lacked the financial foundations to encourage primary enrollments. Finally, much of what investment there was had to be used for water management projects. Anhwei, for instance, "put in more than half of the 93.3 percent that constitutes the productive investment for the five years under the program of capital construction for the erection of water conservancy works" (*Anhwei Jih-pao*, October 1, 1957).

In explaining interprovincial variations in the provision of primary education, economic variables affect not only the government's ability to *supply* such services, they also affect popular *demand* for primary education. In both Shantung and Anhwei, the frequency and scope of natural calamities has meant that villages are frequently uprooted. Dike repair and construction require workers. Both factors made it difficult to assure continuity in school attendance and made it likely that families were more frequently unable to afford the inevitable "sundry expenses" associated with school (*Tachung Jih-pao,* August 17, 1957). While neither officials in Shantung nor Anhwei publicly articulated the close relationship between the stability of rural life and primary school enrollments, Shansi officials did.

> Total enrollment in elementary schools . . . fulfilled only 92.4 percent of the planned goal because of the high mobility of the students as the result of natural calamities in agricultural production (*Shansi Jih-pao,* May 12, 1958).

While it cannot be demonstrated that *provincial-level* political characteristics were important variables in explaining interprovincial variations in the provision of primary education in the 1950s, this should *not* lead one to conclude that political leadership and conscious policy choice is unimportant at the *national* level. As I will discuss below, conscious central political choice concerning taxation strategies, investment allocation, and policy priorities all determine how the economy will interact with the program, as well as determining the long-run configuration of the economy itself. For instance, the economy will variously affect the distribution of primary education depending upon whether it is funded locally or through the national treasury and whether or not the central government subsidizes backward provinces (Lardy, 1975a). Likewise, the decision to invest heavily in one province (e.g., Shansi) to the exclusion of others has demonstrable consequences for the distribution of primary enrollments among provinces.

How well do these generalizations predict the patterns of primary education in the 1970s? Table 1 shows that the four provinces for which we have recent data are performing approximately equally. The hypotheses derived from the examination of the 1950s would suggest that this convergence should have been accompanied by one or more of the following: (1) relatively more rapid increases in GVIO on the part of Anhwei, Shantung, and Hunan than Kwangtung and Shansi provinces; (2) changes in the financing system so as to make schools less dependent upon variable and unequal local revenues; (3) changes in investment policy. Much of this appears to have occurred.

Table 5's GVIO index numbers (column c) clearly demonstrate that the provinces with the lowest per capita GVIO in the 1950s have grown more rapidly than Shansi and Kwangtung, though the absolute gaps probably are still significant. Unfortunately, we have no recent data on provincial-level investment or urbanization. We must assume that those alterations which may have occurred along these two dimensions are consistent with the direction of change in GVIO. The apparent trend toward convergence in the industrial sector, alone, would lead one to predict a narrowing of the differences in primary school enrollment rates between the case provinces.

As predicted, another factor was apparently particularly important in producing the observed convergence in primary enrollment rates among provinces. In 1957, it was announced that "in accordance with the decision of the State Council, the salaries of elementary school teachers, *which have been appropriated from locally raised funds,* will be appropriated from the *National Treasury" (Tachung Jih-pao,* August 17, 1957, emphasis added). One important obstacle to establishing schools in relatively impoverished localities was thereby removed, and we presume this policy was still operative in the 1970s. The effect of this alteration was to make primary school programs less dependent upon variable and unequal local revenues; this has promoted the equalization of enrollment rates between provinces.

We must consider one other factor which would not have been forecast from the 1950s cases. The length of primary education in the early and mid-1970s was, generally, five years, whereas prior to the Cultural Revolution it was six. As ex-Minister of Education Chou Jung-hsin told a delegation of American educators,

> Some of the factors which have made possible the progress in universalizing elementary and secondary education are the simplification of schooling materials and shortening the number of years of schooling (Delegation of the American Association of State Colleges and Universities, 1975, p. 21).

Shortening the number of years in school has made it possible to divert resources from the old practice of keeping fewer students in school longer to the newer system of keeping more students in school for a shorter time. At the same time when the number of years has been reduced, it became increasingly difficult for the provinces which led in the 1950s to further boost their rates. As Orleans (1961, p. 32) explains, "There is a normal slowing up as the proportion in school approaches the school-age population. . . ." One reason for this convergence, then, has been that the indicators are measuring the enrollment levels of a somewhat altered system of primary education. This has favored the "poor performers" of the 1950s (Hunan Provincial Service, February 2, 1972).

To conclude, the factors which affected the levels of primary enrollment in the five provinces during the 1950s still appear to hold. Relatively rapid increases in the GVIO index in the previously low-ranking provinces have contributed to a trend toward convergence; we presume trends in urbanization have paralleled changes in industrial output, but we cannot be certain. Also, the movement toward interprovincial equality reflects changes in financing patterns which have attenuated the link between local financial resources and the provision of primary education. Finally, the length of primary schooling was generally shortened, making it easier for poorly performing provinces to boost enrollment levels. Concisely stated, while economic variables appear to be important determinants of program performance with respect to any given program, and urban areas will always enjoy benefits (especially on the demand side), the shape of the program and the financial relationships governing it are not givens. Political choices concerning which sectors of the economy to emphasize, which provinces to target for investment, levels and kinds of subsidization, length of curriculum, and how to fund a given service, all ultimately determine the pattern of interprovincial performance. The Chinese case, then, suggests that models of strict economic determinism in which political leaders are mere putty in the hands of economic forces beyond their control may need to be modified, as Chenery (1974) and Huntington and Nelson (1976) have suggested.

General Middle School Education in the 1950s and the Mid-1970s

General middle school education has not been nearly as widely available as primary education (compare Tables 1, 2, and 6). In 1975, 3.8 percent of the national population was enrolled in middle schools while about 14.5 percent was enrolled in primary schools. Despite this relatively low enrollment rate, there have been variations in the performance of the five provinces. The following analysis suggests that interprovincial variations in middle school enrollments during the 1950s were very closely tied to rates of urbanization and industrial

Table 5. Selected Attributes for Five Chinese Provinces in the 1970s

	Grain Output/ Capita (in Catties) 1974–75[a]	GVIO/Capita 1973 (in *yüan*)[b]	Index of GVIO, 1972 (1965=100)[c]	Roads: km/1000 km² 1972[d]	Railroads: km/1000 km² 1972[d]
Anhwei	683 rank 3	N.A.	225 rank 3	100.5 rank 4	9.2 rank 3
Hunan	747 rank 1	N.A.	240 rank 2	133.4 rank 2	4.3 rank 4
Kwangtung	684 rank 2	340.55	182 rank 4	84.8 rank 5	2.0 rank 5
Shansi	565 rank 4	N.A.	174 rank 5	126.7 rank 3	12.5 rank 2
Shantung	451 rank 5	253.66	254 rank 1	135.3 rank 1	12.6 rank 1

Source: As indicated in notes below.

[a] All figures have been rounded to the nearest catty. The grain figures for the 1970s, unlike those for the 1950s, *include soybeans.* The foodgrain output data are from "China: Agricultural Performances in 1975," *Research Aid,* ER–10149 (Washington, D.C.: Central Intelligence Agency, 1976), p. 18. The figures displayed are the averages of the per capita figures for two years (1974 and 1975) for each province.

[b] Reported and estimated absolute GVIO figures (in 1952 constant prices) are reported in Robert Michael Field, Nicholas R. Lardy, and John Emerson, "A Reconstruction of the Gross Value of Industrial Output by Province in the People's Republic of China: 1949–73," *Foreign Economic Report,* No. 7 (Washington, D.C.: Department of Commerce, 1975), p. 9.

[c] Field et al. (1975), pp. 34–35.

[d] All road and railroad miles include all lengths of route as of 1972.

growth.[4] For instance, Shansi and Kwangtung provinces (the most urbanized case provinces with the highest average per capita GVIO) had the highest levels of middle school enrollment by 1955 (see Table 6). The apparent association is also demonstrable if statistical tests are run for years for which we have complete middle school enrollment data (1955, 1956, 1957).[5] While one cannot emphasize too strongly the necessity for caution in accepting statistical conclusions based on such a small number of cases, it is clear that future comprehensive performance research on China must take into account the probable relationships between industry, urbanization, and middle school enrollments.

If there was a strong relationship between a province's middle school enrollment level and its degree of urban and industrial development in the 1950s, what factors account for such a relationship and how have subsequent policies dealt with those relationships?

Four reasons suggest themselves: on the demand side, urbanization and industrial expansion created needs for technical and managerial skills. In the more agrarian provinces, such requirements were fewer. Also, in agricultural settings, a peasant family simply could not forego a child's labor power for the length of time necessary for the child to receive a middle school education. As well, middle schools were generally located in central places of some size. An urbanized provinde simply afforded easier access to more people than was the case in rural units.[6]

On the supply side, the relatively urbanized provinces had greater resources with which to provide middle school facilities. An important asset was the "state enterprise" which could establish attached secondary technical and general middle schools. A provincial leadership which had a concentration of state enterprises had an advantage in achieving higher rates of enrollment. Finally, in Kwangtung, the availability of resources provided by the overseas Chinese came into play with respect to *both* industrialization and middle school education.

> In 1956, for example, Overseas Chinese investment was reported to have accounted for 10 percent of the total local industrial investment

Table 6. Enrollments in General Middle School in Five Chinese Provinces, 1949–1975 (Percentage of Provincial Population Enrolled)

	1949	1952	1953	1954	1955	1956	1957	1960	1965	1975
National Average	.19	.43	.49	.59	.63	.82	.98	N.A.	N.A.	3.8
Anhwei					.32	.39	.63			
Hunan		.36			.45	.57	.56	.73	.98	3.36
Kwangtung	.39	.67	.71	.80	.82	1.12	1.35			
Shansi	.076	.40			.58	.86	.99			
Shantung	.08				.33	.42	.50			

Source: All enrollment data from which these statistics were generated are from the following Chinese provincial newspapers: *Anhwei Jih-pao, Hsin Hunan Pao, Nan-fang Jih-pao, Shansi Jih-pao, Tachung Jih-pao, Ta Kung Pao, Tsingtao Jih-pao,* and *Wen Hui Pao.* The figures for 1965 and 1975 were obtained from *Foreign Broadcast Information Service: Daily Report, People's Republic of China,* No. 99 (1975), p. H2. The specific citation for each figure will be provided upon request.

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[4] Zwick (1976) finds that this is true in the Soviet Union.

[5] When the data were analyzed as interval-level data, the strength of positive association between the percent of population living in urban places (Table 3, column c) and middle school enrollment levels, as measured by Pearson's R, varied from .9 (significant to the .01 level) to .85 (significant to the .03 level). When the data were expressed as rank orders for each year, the Tau B's varied from .8 to .4. Average per capita GVIO (Table 3, column b) is positively correlated with the level of middle school enrollment. The strength of association as measured by Tau B varied from .6 to .2 and when measured by Pearson's R varied from .81 (significant to the .04 level) to .75 (significant to the .06 level).

[6] While a province with a greater percentage of its population in urban places will have higher rates of middle school enrollment, one must note that the province is not an ideal unit for which to measure urbanization. Regional urban systems cut across provincial boundaries. For instance, it was an administrative and political decision that removed Shanghai from the jurisdiction of Kiangsu Province and removed Peking and Tientsin from the jurisdiction of Hopeh. Likewise, a city such as Nanking, which lies near a provincial boundary, may service hinterlands that are located in neighboring provinces.

> in the two provinces [Kwangtung and Fukien]; and in the same report it was stated that 23 percent of all middle school students in Fukien were enrolled in schools established with Overseas Chinese capital (Fitzgerald, 1972, p. 125).

While the latter half of this report referred only to Fukien, the situation was almost certainly similar in neighboring Kwangtung. How were these variables manifested in the provinces under examination?

In 1949, Shansi had a smaller per capita GVIO than both Kwangtung and Shantung (see Table 4). Shansi did less well than either in middle school enrollments (see Table 6). However, once Shansi was designated an "industrial keypoint," its GVIO per capita rose rapidly in the 1952–1957 period (see Table 4), as did its middle school enrollment rate. Kwangtung, which had the second highest rate of growth of per capita GVIO during this period, had the second most rapid rate of middle school expansion.

Levels of middle school enrollment apparently did not depend on agriculture during the 1949–1957 period. The Shansi and Hunan cases offer striking indication of this. While Shansi was forging ahead industrially and its rate of urbanization was comparatively high, it did poorly in per capita foodgrain production (Table 3, column a), ranking only ahead of Shantung. Conversely, while Hunan led the five provinces in per capita foodgrain output,[7] it did only marginally better than Shantung and Anhwei in middle school enrollment. We tentatively conclude that urban development and industrial growth are the most powerful factors affecting the provision of middle school education. This suggests that if policy makers in China wish to achieve interprovincial equality in access to middle school education, they will be most successful by promoting a program of equalization of urban and industrial growth among provinces. Lardy (1975a) argues that the fiscal system has been designed to accomplish just this.

Unfortunately, due to the absence of middle school enrollment data for the 1960s and 1970s for all of the provinces except Hunan (see Table 6), we are unable to determine whether or not a trend toward equalization of middle school enrollment rates developed during that period. However, given the apparently close links between urbanization, average per capita GVIO, and middle school enrollment levels in the 1950s, it is probable that the relatively more rapid growth in GVIO in Hunan, Anhwei, and Shantung during the 1965–1972 period (see Table 5, column c) would have produced some reduction in disparities among the provinces, though one would predict that it would take much longer to overcome inequalities in middle school enrollments than was the case with primary education. This, however, is a hypothesis which must be tested when, and if, appropriate data become available.

One further piece of recent evidence links rates of middle school enrollment to the degree of urbanization for the 1970s. In April 1975, visiting American educators were told by a Soochow official that, "we have the principle of enrolling the students who live nearby the schools. . . . Nearly 100 percent of the primary school pupils go on learning in the middle school. . ." (Delegation of the American Association of State Colleges and Universities, 1975, p. 29). If only a small percentage of primary school graduates (nationally about one in 3.5) could go to regular middle schools in 1972 (Lampton, 1978), and one finds that the percentage of urban children going to such schools is much higher, it means ipso facto that there is a marked urban bias in the availability of such education.

One would predict that *if* the Chinese leadership continues to promote urban/industrial growth in presently less developed provinces, the resulting changes should produce increasing equality in middle school enrollments among the provinces, though, within provinces, urban areas will remain advantaged. This suggests that the conscious choices of political leaders in Peking, where allocations to the various provinces are made, are important. If they continue to follow policies of equalizing urban and industrial growth among provinces (Lardy, 1975a and 1975b), they will then probably promote equalization of middle school enrollments. Conversely, decisions to focus industrial investment and urban growth in selected areas would have the reverse effect.

The Origins of Interprovincial Inequalities in Hospital Beds in the 1950s and 1970s

As with secondary education, relatively urbanized provinces were able to provide more hospital beds per thousand population than the other provinces for which we have data (Table 7). Comparing the performance of the four provinces for which we have information with their aggregate attributes, the province which was almost twice as urbanized as the others did approximately twice as well as the other provinces in providing hospital beds. Foodgrain output per capita was apparently not a major factor. If agriculture had been a more salient factor, one would have expected Hunan (the leading per capita foodgrain producer) to have done significantly better than Shantung, the province which ranked last (see Table 3, column a).

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[7] Wiens (1975, p. 93) notes that Hunan's yields probably dropped in the 1952–1957 period.

While data on the number of hospital beds are exceedingly fragmentary, the appearance of a positive association between percent of provincial population residing in urban places and level of hospital beds holds if statistical tests are performed. Of course, the small number of cases and the spotty data assure that the findings can only be tentative.[8]

Several factors account for the apparently close relationship between urbanization, industrial output, and hospital bed availability. First, effective demand for hospital facilities is greater in urban areas.

> In a developing country distance is a critical determinant of medical care, and it is widely realized that only those close to a medical unit can derive the full benefit from its services. . . . The average number of outpatient attendances per person per year will be seen to halve itself about every two miles. . . . The number of persons per square mile determines the possibility of providing them with medical care nearly as profoundly as does the distance of a patient's home from a medical unit (King, 1966, pp. 2:6–2:8).

Also, the financial terms of access to curative facilities favored areas with high concentrations of workers covered by the 1951 Labor Insurance Regulations (Kallgren, 1969). Starting in 1951, many employees of large and/or critical industries were covered by labor and/or medical insurance (Donnithorne, 1967, p. 213). For those with medical coverage, most financial barriers to receiving medical care had been removed. Also, by at least 1957, medical facilities were receiving periodic allotments proportional to the number of workers for which the hospital was responsible (*Weekly Information Report on Communist China*, May 26, 1958, p. 37). These benefits and the method by which hospitals were paid linked demand for hospital services to urban areas.

Additionally, workers were concentrated in cities where they could constitute a political force, to a limited extent. There was continual tension between the All China Federation of Trade Unions, its membership, and the regime over the level of material and "fringe" benefits. The stress was particularly acute in 1956 and 1957 in provinces such as Shansi and Kwangtung which had high concentrations of workers. At the Second Congress of the T'aiyuan Municipal Party, Party leaders observed that

> many people, and even some Party members and cadres, were not fully aware of the important significance of the production-increase and economy campaign. These people . . . insisted on the improvement of living standards for individuals (*New China News Agency*, February 2, 1957, p. 16).

If the relationships suggested above are correct, there should be an association between the provision of hospital facilities and the percentage of provincial population with labor

Table 7. Hospital Beds Per Thousand Population in Four Case Provinces, 1949–1957 (Includes Hospital and Sanatoria Beds)

	1949	1950	1951	1952	1953	1954	1955	1956	1957
National Average	.15			.31		.41	.45	.52	.56
Anhwei	.0061					.28	.23	.31	.33
Hunan				.33					.39
Kwangtung						.37	.44	.68	
Shantung	.06							.32	.32

Source: All hospital bed data from which these statistics were generated are from the following provincial newspapers: *Anhwei Jih-pao, Hsin Hunan Pao, Nan-fang Jih-pao, Tachung Jih-pao,* and *Tsingtao Jih-pao.* Aggregate national level data are from *Ten Great Years,* p. 220. Precise documentation will be provided for each figure upon request.

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[8] The strength of positive correlation between percent of population in urban areas (Table 3, column c) and a province's provision of hospital beds per thousand population (for the years 1956 and 1957), when measured by Pearson's *R*, varied from .99 (significant to the .002 level) to .66 (significant to the .26 level). When the data were expressed as rank orders for each year, the Tau B's varied from 1.0 to .33. As well, I suspect, and statistical tests confirm in all but one case, that average GVIO per capita (Table 3, column b) is positively associated with hospital beds per thousand population.

insurance.[9] The available data are consistent with the presumed set of relationships (Table 8). In 1956, Canton City had more insured workers than the entire province of Anhwei had that year and Shantung had the following year. Kwangtung ranked ahead of all the provinces for which data are available.[10] The trend seems clear; Anhwei and Shantung had weak economic and political demand for hospital services in the 1950s.

In identifying the probable sources of interprovincial inequalities in the availability of hospital beds during the 1950s, our investigation suggests that the twin (and highly correlated) factors of urbanization and the level of per capita GVIO are most important and that both of these variables are reflected in the number of insured workers (see Table 8). This finding implies that hospital bed inequalities among provinces could be reduced by a policy of equalization of industrial and urban growth. Lardy's work (1975a and 1975b) suggests that the central authorities in China have adopted this approach, and the relatively rapid growth of GVIO in Anhwei, Hunan, and Shantung (see Table 5, column c) would seem to reflect this policy. Once again, while factors such as degree of urbanization and per capita GVIO are linked to performance, leadership choice at the central level can play a critical role in shaping the long-term structure of the economy and, thereby, ultimately affect the distribution of hospital beds among provinces. Of course, equalizing urban-industrial growth between provinces is an arduous task, and one would predict some significant lag between changes in patterns of industrial and urban development and alterations in the distribution of hospital beds.

Do these generalizations hold into the 1960s and 1970s? Concisely, we cannot be certain because the Chinese have released almost no provincial-level data on hospital beds since 1960. However, a few inferences may be drawn. Because Cultural Revolution criticisms of the medical system were aimed at the concentration of medical resources in urban areas, it is likely that before 1966 rural provinces continued to do substantially more poorly than relatively urban ones. Even today, the largely urbanized provincial-level units of Shanghai and Peking have hospital resources far in excess of those to which they would be entitled if all resources were distributed equally (Lampton, 1974, p. 9).

The persistence of interprovincial (and urban-rural) gaps explains the post-Cultural Revolution alterations in the health system. Indeed, it is the apparently strong relationship between urbanization, industrial output, and hospital beds which led central policy makers in Peking to try to find an alternative to a hospital-based system. Since 1968, a "new" health care delivery system has taken shape: the "cooperative health care system" (*ho-tso chih-liao chih-*

Table 8. Number of Insured Workers, by Province, 1951–1957

Province	1951	1952	1953	1954	1955	1956	1957
Anhwei					62,500 (.20)	63,300 (.19)	
Hunan		50,000 (.15)			141,000 (.41)	283,000 (.80)	280,000 (.78)
Kwangtung			78,333 (.23)	94,000 (.27)	150,400 (.42)		
Canton City*				37,398 (2.33)	46,000 (2.87)	64,000 (3.43)	
Shantung							51,000 (.09)

Source: The absolute numbers of workers with insurance were obtained from the following provincial newspapers: *Anhwei Jih-pao, Hsin Hunan Pao, Nan-fang Jih-pao, Tachung Jih-pao,* and *Wen Hui Pao.* Precise documentation will be provided upon request.

Note: The numbers in parentheses are the percentage of provincial population with labor insurance.

*Non-provincial-level unit included for informational purposes.

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[9] We chose the number covered by labor insurance instead of medical insurance because the data are better for the former than the latter.

[10] See Table 8. There is an ambiguity in the coverage of the 1953, 1954, and 1955 figures for Kwangtung, but this does *not* affect the analysis.

tu). Two features of this system are particularly significant: first, production brigade health clinics and commune medical clinics have been established.[11] These clinics employ paramedics, "barefoot doctors" (*ch'ih-chiao i-sheng*), who provide diagnostic, preventive, and limited curative services. Those cases requiring additional treatment are supposed to be referred to more comprehensive facilities. Secondly, these services were meant to be financed locally using the resources of cooperative welfare funds and individual contributions; both the welfare funds and individual incomes are very dependent upon local agricultural well-being. In contrast to a health system based on hospital care, then, the presence or absence of these medical programs is much more a function of the local agricultural situation than trends in the urban-industrial sector. Indeed, cooperative health programs have had to deal with highly variable local revenues because of their close ties to the local agricultural economy.

The Determinants of Cooperative Health Care Availability in the Late 1960s and the Mid-1970s

The capacity of production brigades to establish and maintain cooperative health programs is intimately tied to local agricultural production; the financial provisions described above make it inevitable. The size of commune and brigade welfare funds is directly tied to local agricultural output. These funds receive a fixed percentage of cooperative receipts. Also, peasant incomes are linked directly to agricultural production; as their incomes drop, their ability to pay for medical care declines.

Ideally, in order to demonstrate rigorously the links between local (commune and brigade) agricultural production and the availability of cooperative health programs at these same levels, one would like local production and performance data. Unfortunately, such systematic statistics are unavailable. Consequently, we have had to assume that program and production increases and decreases occurring at the *provincial level* reflect those trends and relationships which we are unable to observe directly at the local level. Using such reasoning, the analyst would predict the following sets of relationships between provincial (aggregate) foodgrain output and the availability of cooperative health programs at the provincial level. *Increases in the percentage of a province's production brigades with cooperative health programs should be associated with increases in aggregate grain production for that same province the preceding year. Conversely, declines in a province's aggregate foodgrain output one year should be followed the next year by a decline in the percentage of that province's production brigades with cooperative health programs.* The reason for the one-year lag between grain production and program performance is that one year's agricultural increases or decreases are not fully felt until the following year. Quantitative and qualitative data for our case provinces, and national-level aggregate data, all support these presumed relationships.

To test these linkages during the 1971–1975 period, we divided Chinese provinces into those which achieved aggregate increases in foodgrain output and those which experienced declines (see Table 9). Table 10 provides data on each province's performance in providing cooperative health programs. There have been variations in program level within and between provinces.

In Tables 9 and 10 there are 17 provinces for which we have both the foodgrain production and program performance data necessary to test the hypothesized relationships. (These provinces are designated by asterisks in Table 9.) In 12 of the 17 cases, provincial foodgrain increases for one year were followed in the next by increases in the availability of cooperative health programs within these same provinces, as predicted. In 2 of the 17 cases, documented provincial foodgrain declines were followed the next year by decreases in the availability of cooperative health programs within those provinces, as predicted. Only 3 cases deviated from the hypothesized outcome. One of these cases (Anhwei) actually confirms the hypothesis when understood. In 1972, provincial grain output in Anhwei rose slightly, but the increase was less than the expansion in population; there had been a per capita foodgrain decline. This suggests that per capita foodgrain production would be a better indicator. Unfortunately, we lack sufficient quantitative data to systematically use that variable. Conservatively speaking, then, 14 of 17 cases fit the hypothesis. The odds that this occurred by chance are less than 2 percent.[12]

Examining the case provinces, we find that these aggregate relationships hold, except for

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[11] Production brigades are units of rural population, production, and administration approximately the size of a natural village, though this varies.

[12] We applied the chi-square test. For an explanation see Blalock (1972, pp. 275–85).

Anhwei in 1971–1972. For instance, Kwangtung suffered grain production declines in 1972 and 1975. The years immediately following witnessed reductions in the percentage of production brigades providing cooperative health care. Conversely, both Shantung and Hunan experienced increases in foodgrain production in 1975; the following year both provinces achieved increases in program availability.

The close link which appears to exist between agricultural production and cooperative health care in the aggregate provincial-level data is also apparent in the qualitative information coming from lower-level units within the case provinces. For example, some basic-level cadres in Hsin County, Shantung, were denounced for having let a cooperative health program collapse for allegedly economic reasons. They reportedly

> spread the word that since economic conditions here were so poor, and commune members had no money, the medical cooperative system could not exist any longer (Shantung Provincial Service, March 20, 1974).

While it is clear that agricultural prosperity is essential to the survival and growth of cooperative health programs, can it be argued that provincial-level leadership stability is a significant factor affecting a province's capability to implement these programs? Although provincial leadership data are defective in several re-

Table 9. Provincial Increases and Decreases in Foodgrain Production, 1971–1975

1971	1972	1973	1974	1975
	Provinces with a decline in foodgrain production over the previous year			
Kwangtung	Honan		Anhwei	Chekiang
	Hopeh		Sinkiang	Honan
	Kwangtung*		Szechuan	Hopeh
	Kweichow			Inner Mongolia
	Sinkiang			Kiangsi
				Kiangsu
				Kweichow
				Kwangtung*
				Ninghsia
	Provinces with an increase in grain production over the previous year			
Anhwei*	Anhwei*	Anhwei	Hopeh	Anhwei
Honan	Fukien*	Chekiang	Hunan	Fukien
Hopeh*	Kansu	Fukien	Hupeh	Heilungkiang
Shansi	Kwangsi*	Heilungkiang	Kiangsu	Hunan*
Yunnan*	Shantung	Honan	Kwangtung	Hupeh*
	Tsinghai*	Hopeh	Kwangsi	Kansu*
		Hunan	Liaoning	Kirin*
		Hupeh		Kwangsi
		Inner Mongolia*		Liaoning
		Kiangsu		Shansi
		Kirin*		Shantung*
		Kwangsi*		Shensi
		Kwangtung		Sinkiang
		Kweichow		Tibet
		Liaoning		Tsinghai
		Shantung		
		Shensi		
		Sinkiang		
		Szechwan		
		Yunnan		

Source: Provincial performance in foodgrain output was determined from "China: Agricultural Performance in 1975," *Research Aid* (Washington, D.C.: Central Intelligence Agency, 1976), p. 18; *Foreign Broadcast Information Service: Daily Report, People's Republic of China* (1972–1973); *China News Summary*, Nos. 411, 447, 452, 454, 475, 476, and 497; *Survey of China Mainland Press* (American Consulate General: Hong Kong), No. 5041 (December 27, 1971), p. 15. Precise documentation for each province will be provided upon request.

*A province for which both grain data and the two appropriate consecutive years of cooperative health care performance data are available.

spects,[13] the data acquired in the course of this study would lead one to conclude that there is little discernible relationship between program performance and the stability of provincial leadership. For instance, Kwangtung had more shifts in first party secretaries during the 1972–1976 period than any of the other case provinces.[14] While one might hypothesize that such instability would disrupt administration and thereby impair performance, Kwangtung did well in providing cooperative health programs. Conversely, Hunan had greater continuity of first party secretaries in the 1970–1976 period, with Hua Kuo-feng providing continual leadership. Yet Hunan has done less well in establishing continuity in its cooperative health program than Kwangtung. This suggests that while the central leadership in Peking makes critical choices concerning funding and investment strategies, subsidies, and program shape, the choices made by provincial leaders are not demonstrably important factors in determining provincial performance.

Conclusions

Three theoretically important conclusions emerge from this study, findings relevant to developmental theorists, policy analysts, and sinologists. First, there is no single economic variable which accounts for the observed interprovincial variations in performance across all

Table 10. Percentage of Production Brigades with Cooperative Health Programs, by Province, 1969–1976

Province	1969	1970	1971	1972	1973	1974	1975	1976
Anhwei			70	68.7	50			
Chekiang			71					
Fukien				80	74		80	87
Heilungkiang							94	
Honan							75	
Hopeh			80	80.6				
Hunan		97.4		Feb. 90 June 50			79	83
Hupeh					83		80	93
Inner Mongolia					33*	50	71	
Kansu			80	85			85	90
Kiangsi				93				
Kiangsu		June 80 Sept. 95						95
Kirin					68*	90*	90	
Kwangsi				66.7	70	90		95
Kwangtung	50	87		100	80		97.3	93
Kweichow				67.5				
Liaoning								90
Ninghsia							80	
Peking					75			
Shansi			85					
Shantung			50			53	85	90
Shensi		84						86.4
Sinkiang		70			90			
Szechwan				80				
Tientsin							95	
Tsinghai				42	70	50	70	
Yunnan		90		100		80		

Source: These figures were obtained from provincial radio broadcasts monitored by *Foreign Broadcast Information Service: Daily Report, People's Republic of China* (1973–1976), *Summary of World Broadcasts* (1969–1974), and *The Chinese Medical Journal* 1, No. 1 (1975), pp. 3–12. One must be critical of figures which allege participation rates of 100 percent. However, the rates of each province taken as a set are, in all probability, accurate indicators of trends in that province. Precise documentation for each statistic will be provided upon request.

*This statistic is for a model area only; the province-wide rate was probably lower, if anything.

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[13] We lack data on the middle levels of the provincial bureaucracies.

[14] Chao Tzu-yang, Ting Sheng, and Wei Kuo-ch'ing.

of the policy areas examined. Agricultural growth is a precondition to success in cooperative health care (as it is presently funded) and not particularly helpful in obtaining high levels of primary or middle school enrollments, or hospital facilities.[15] While the specific linkages between these particular programs and the economy are peculiar to China, it suggests that policy analysts in the Third World setting (where there is a great bifurcation of the economy into rural and urban sectors) must be sensitive to the variable linkages between specific programs and specific sectors of the economy.

Second, the above finding leads one to refine and revise the conventional wisdom of many developmental and policy studies. Policy studies emerging from rather wealthy industrial societies, such as those of Dye (1966) and Wilensky (1975), have generally concluded that program performance is most closely tied to economic variables (usually per capita GNP). This finding, in turn, has fit nicely with the prevailing developmental notion that if one emphasizes economic growth, other fundamental values such as equality and participation will automatically be realized in its wake. As Huntington and Nelson (1976) and Chenery (1974) have begun to argue, both trends have tended to negate the importance attached to leadership choice, making it appear that the only job of the political leader is to maximize aggregate economic growth. Such a conclusion is not warranted by this study.

Central policy makers in China (as elsewhere) face an enormous array of difficult choices. As this study has shown, equality of education and medical services among Chinese provinces is affected by the leadership's choice of financing arrangements, by choices concerning which areas to designate as "key points" for investment, by decisions about whether to emphasize investment in agriculture or industry, by decisions about whether or not to subsidize various programs and provinces, and by the particular provisions and configurations of programs themselves. For instance, the decisions to invest more heavily in agriculture, to equalize urban development, to make localities less dependent upon variable local financing for primary schooling, and to move away from a strictly hospital-based system, seem to have produced greater equality in the distribution of primary education and basic medical services. To be specific, designating Shansi an industrial "key point" in the early 1950s provided the basis for that province's rapid expansion of primary and middle schools. Similarly, emphasizing industrial growth in Shantung and Anhwei in the 1960s and 1970s appears to have provided the basis for their recent gains in primary education.

The critical point is that while leaders can never eliminate economic constraints, their political choices do shape the long-term structure of those constraints and, in the short run, determine how those economic variables will manifest themselves through concrete programs. Most importantly, leadership policy choice reflects value choice. A central question for the future is, will the value emphasis of the pre-1977 period continue in the post-Mao era?

Third, one need not assume that interprovincial variation in program performance reflects significant discretionary authority available to provincial leaders. The layering of authority is such that the decisions which most affect levels of performance are generally made in Peking. This suggests that provincial leaders have a stake in influencing national policy makers in at least two respects: first, provincial leaders who wish to maximize the performance of "their" provinces would be well advised to attempt to encourage central authorities to adopt programs which they believe are consistent with the constraints imposed by each province. Secondly, provincial leaders should push for more state investment and they should marginally prefer those forms of investment which will most strengthen the provincial economy in those areas most important to achieving priority objectives. A major question for further research is, how have these presumed interests been concretely articulated and do Chinese leaders define their interests in this way? Certainly, what little information we do have on the budgetary process in China suggests that provincial leaders do consciously and openly seek to minimize the outflow of provincial resources and maximize the allocations made to their respective provinces (Donnithorne, 1967, pp. 387–88; also, Oksenberg, 1969).

To conclude, the provision of education and health services in China, as elsewhere, is shaped by economic variables. However, policy makers need not only to know this, but also to understand which program is affected by which sector of the economy and how. These complex relationships provide national-level policy makers with important areas of choice. These choices, in turn, are guided by fundamental values, and the interaction of local and national political leaders. The task of future research is to identify more precisely how much latitude for choice in fact exists, why certain values come to dominate any given decision process, how various political actors seek to influence these critical choices, and what are the long-term consequences of present decisions.

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[15] This study has not sought to establish generalizations designed to predict the performance of border and minority regions. Teiwes' (1971) finding that defense areas are unusually stable, and my own findings (1978) that by 1957 these areas did unusually well in providing hospital beds, suggests that the small populations involved, along with leadership concern for these localities, has altered their performance patterns.

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Coalitional Behavior among the Chinese Military Elite: A Nonrecursive, Simultaneous Equations, and Multiplicative Causal Model*

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At least three competing proto-theories of Chinese military coalitional behavior (those of William Whitson, Harvey Nelsen, and William Parrish) have been proposed. This study attempts to: (1) reformulate these proto-theories into testable mathematical models, (2) test these reformulated models empirically, and (3) suggest an alternative theory of Chinese military coalitional behavior. I use causal modeling techniques to reformulate these proto-theories into mathematical ones and gather data on 423 members of the Chinese military elite to test the zero-predictions and structural equations of each model. The findings indicate that all three proto-theories may be misspecified theoretically. I then propose an alternative theory, which integrates features from all three proto-theories. This theory argues that members of the Chinese military elite form coalitions according to a set of affective ties and shared professional interests. This set of relationships is stated in mathematical terms, and the mathematical predictions deduced from it fit empirical data.

The study of factionalism in the Chinese People's Liberation Army (PLA) has reached the initial stage of model building where competing proto-theories have been proposed. The three most important proto-theories of PLA factionalism are those of Whitson, Nelsen, and Parrish (Ting, 1975). These proto-theories have made enormous contributions to Chinese studies, but the debate about PLA factionalism cannot be resolved until these proto-theories are restated in empirically testable forms. Also, many concepts in these proto-theories, including the concept of factionalism, are not defined and measured. Finally, these existing proto-theories are often bivariate, which means that spurious relationships, and complex causal relationships in general, cannot be inferred and tested. When concepts in bivariate models are defined and measured, the best one can arrive at mathematically from these models are non-theoretical covariance statements where no causal asymmetries are implied (Blalock, 1969).

The objectives of this study are to (a) define key concepts of PLA factionalism, (b) reformulate these proto-theories of PLA factionalism into testable mathematical models, and (c) integrate these proto-theories into a single multivariate, multiplicative, and nonrecursive causal model of coalitional behavior among members of the Chinese elite that can be generalized to the PLA case. By so doing, I hope to contribute to theory verifications, modifications, and constructions in the field of PLA studies.

Two disclaimers need to be made at the outset. First, translations of proto-theories into mathematical ones require the process of abstraction, since mathematics is a deductive system involving abstract symbols. However, mathematical abstractions cannot be equated isomorphically to "realities." Therefore, my findings cannot be equated isomorphically to the original proto-theories too. Second, a corollary disclaimer is that this study is not an attempt to reject any one of the proto-theories of Chinese elite behavior; rather, it is to show their theoretical interdependence. Correspondingly, the theme here is that Chinese military politics can be explained best by integrating and modifying certain features from all three proto-theories.

Key Points of Contention Among the Proto-Theories

From its inception in 1927 to its final triumph over the Nationalists in 1949, the

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Chinese Red Army consistently faced enemies with superior fire power. Thus, the Red Army's strategy and organizational principle during that time were non-positional military engagements and a decentralized command.[1] As a result, five independently commanded branches of the Red Army had evolved by 1947 (Whitson, 1973).

In 1947, these five branches were organized officially into five field armies, and the Red Army was renamed the People's Liberation Army. Each field army was assigned the task of capturing one region of China during the final phase of the Chinese Civil War from 1947 to 1949. The field army system was abolished in 1954 after the Korean War. In its place, military regions were set up. William Whitson contends, however, that old field army loyalties among members of the PLA elite dominated PLA politics even after 1954. In fact, he argues, post-1954 PLA politics can be analyzed best in terms of field army factions competing for patronage and power (Whitson, 1968, 1973). This model of PLA factionalism is commonly called the "field army model."

The field army model is disputed by Harvey Nelsen and William Parrish. Nelsen's position is that field army ties among the PLA elite members were no longer an important independent variable of Chinese military politics *after* 1967. In 1967, the PLA was ordered by the central command to intervene in the Cultural Revolution on behalf of the "revolutionary left" (Nelsen, 1972). According to Nelsen, many regional PLA elite members refused to carry out the central order.[2] A series of open as well as proxy conflicts between the central and the regional PLA forces followed the central order.

If the field army model is correct, Nelsen argues, then the central and regional PLA elite members should not have clashed so intensely in 1967, since many of them had served in the same field armies. The intensity could have developed only if a conflict of *interests* between the members of the central and the regional PLA elites had overshadowed their field army ties. He argues that central PLA elite individuals were interested in carrying out the order to support the revolutionary left. However, the *target* of the revolutionary left included many regional PLA elite members who were interested in creating pockets of local power. Thus, for members of the regional elite to carry out the central order, many of them would have committed political suicide.

The key point of contention between Whitson and Nelsen is whether field army ties can be considered as a valid tool for explaining *post-1967* PLA politics; Whitson argues that it can, and Nelsen argues the opposite. Both agree that field army ties had played an important role prior to 1967. It is interesting to note that Nelsen does not answer *why*, all of a sudden, field army loyalty was abandoned by members of the PLA elite in 1967. I shall comment on this later.

William Parrish (1973) also argues that field army ties were no longer an important factor in PLA politics during and after the Cultural Revolution. Parrish feels that PLA politics can be analyzed best by what I have termed elsewhere a "bureaucratic model of PLA politics" (Ting, 1975). The behavior pattern of members in a bureaucracy is not constant, Parrish argues, but is determined by the presence or absence of a crisis condition in the bureaucracy. Unfortunately, he does not define "bureaucracy" and "crisis." Thus, in the PLA, which by implication is a bureaucracy, routinization of decision making should be the norm during a non-crisis period. During a crisis, rules and regulations of a bureaucracy should break down, and decision-making processes in the bureaucracy will orient themselves toward personalities.[3]

Parrish (1973) attempts to disprove the field army model empirically by demonstrating that field army ties had little effect on PLA politics during the Cultural Revolution. In so doing, Parrish appears to support Nelsen's model, but his (implied) theory predicts that personalities—such as loyalty to field army leaders—should have dominated PLA politics during the

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[1] There were exceptions to this strategy; the Hundred Regiments Campaign by the Red Army against the Japanese was a good example.

[2] Evidence available seems to indicate that refusal to obey the central order was not limited to the regional PLA elite members alone. Many central elite members argued among themselves as to whether they should support the revolutionary Left too. For a detailed discussion, see Ting (1976). As I demonstrated, intra-central conflicts might have resulted, in part, from old field army ties of the central PLA elite members.

[3] Murray Edelman defines a crisis as a mode of perception and action by a specific group of actors for the purpose of restructuring politics. More specifically, a crisis may be precipitated semantically by groups who wish to respond to an alleged threat, or it may be precipitated by the seizure of an opportunity for enhanced income or power by deprived groups. See Edelman (1977) for further discussions.

Cultural Revolution crisis period. Of course, it is pertinent to restate that the bureaucratic model is only my interpretation of Parrish's view, and Parrish does not present a "PLA model" per se.

Conceptual Definitions, Bivariate Models, and Generalization

Before these verbal theories can be reformulated into testable mathematical models, two theoretical issues should be raised and clarified. First, what is *factionalism*? Second, are these existing proto-theories time-space specific?

Conceptual Definitions. Factionalism is not defined in any of the proto-theories. Consequently, it is possible that Whitson and Nelsen are discussing different *categories* of coalitional behavior when they use the same concept of factionalism. Following game theorists, I shall define coalitional behavior as the decision of a group of actors to coordinate their choices into common strategies (Swaan, 1973). In the case of Chinese politics, the term "to coordinate" need not imply *formal* association among members of a coalition for the purpose of deriving common strategies, nor need it imply *formal* membership (more on this later). In fact, *organized* elite coalitions cannot exist in a communist state, given its ideology and party-state organizational structure (Skilling and Griffith, 1971). This does not mean that elite coalitions cannot exist in communist states, only that such coalitions take different forms from coalitions in multiparty states (Skilling and Griffith, 1971; Ting, 1976).

According to the Whitson model, coalitional *behavior* in the PLA can be defined as informal efforts by the PLA elite members to recruit, promote, and defend the positions of old field army associates and their protégés. I shall call this type of coalitions based on affective, friendship networks among members of the PLA elite as "factions," and its politics as "factional politics."

Coalitional behavior in the Nelsen model can also be reduced to patterns of elite recruitment, retention, and promotion in the PLA. However, to Nelsen, these personnel-decision patterns are determined by the PLA elite members' *current* professional interests and affiliations; that is, whether they are members of the central elite or the regional elite. I shall call coalitions based on professional interests "interest groups," and its politics "interest group politics."

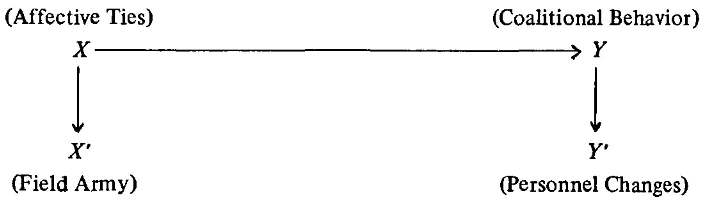
Quite obviously, there are major structural and functional differences between interest groups in multiparty parliamentary states and in communist states. These differences are discussed elsewhere, and need not be documented here (Skilling and Griffith, 1971, and Ting, 1976). There is, however, one major similarity; both perform the function of *interest aggregation* in their respective political systems.

The Problem of Theory Generalization. Social science theories are "stochastic," but physical science theories are "deterministic." This assertion, familiar to all social scientists who are interested in theory building, is only partially correct. For example, quantum theory, in the forefront of theoretical physics today, is statistical and non-deterministic in nature. The stochastic nature of social science reality does not mean that social science phenomena are, therefore, unique occurrences. Thus, the ability to generalize temporally and spatially still should be a criterion of good theories in social sciences.

It is not clear whether the existing verbal theories of PLA coalitional behavior are meant to be general theories. I suspect that they are not, since their variables are only empirical indicators of unmeasured concepts (Costner and Leik, 1964). One distinction between indicators and theoretical variables is that the former are often time-space specific, and the latter are not. I will discuss later the practical effect of having only time-space specific models in PLA research. For now, I shall present a technical argument to demonstrate the weakness of indicator models as conceptual guides.

There is a growing literature on measurement errors in sociology and econometrics, which in part deals with the relationships between indicators and theoretical variables (Blalock, 1964, 1969; Costner, 1964, 1969; Heise, 1969; Johnston, 1972; Wiley and Wiley, 1970). Indicators are the empirical manifestations of theoretical concepts; they are the measurable parts. For example, the concept of status cannot be measured directly, but can be measured indirectly by such indicators as job prestige, education attainment, and income.

Unless the correlations between the indicators and the theoretical concepts they are intended to measure are high, the empirical correlation coefficients we obtain usually will be *attenuated*. Mathematical treatments of this attenuation effect are numerous and excellent in sociology (Blalock, 1964, 1975); therefore, I shall not deal at length with the mathematical aspect of this effect. An example of the attenuation effect for bivariate models will be more appropriate and will suffice here. Take the field army model as an example, which, at the *theoretical* level, states that informal, friendship associations among members of the PLA elite (a theoretical concept) cause their coalitional behavior (another theoretical concept). We *test* this model by using field army affiliations of PLA elite members as the indicator for the independent variable and elite members' purge and recruitment patterns as the indicators for the dependent variable. Using X and Y as the theoretical concepts and X' and Y' as the indicators, we can draw the following causal diagram:



Source: Derived from William Whitson (1969), "Field Army in Chinese Politics," *China Quarterly* 37:1–23; William Whitson (1973), *The Chinese High Command,* New York: Praeger.

Figure 1. Unmeasured Concepts, Measured Indicators, and Correlation

Note that no model specification error is assumed in Figure 1. Even so, the correlation coefficient we obtain empirically (that is, $r_{X'Y'}$) may be attenuated. This is because $r_{X'Y'} = r_{XX'}r_{XY}r_{YY'}$ in Figure 1. Therefore, $r_{X'Y'}$ will be *smaller* than r_{XY} *unless* $r_{XX'} = r_{YY'} = 1$.[4] However, if either $r_{XX'}$ or $r_{YY'}$ equals zero, then the correlation $r_{X'Y'}$ will also be zero.[5] But R^2_{XY} here may range from 0 to 1.

The implication here for research is that, for bivariate models *containing only indicators,* the degree of attenuation in the correlation coefficient is unclear, since the theoretical concepts themselves are not specified. If the theoretical concepts in bivariate models are defined, then the correlations between the two concepts and their indicators still need to be high in order for the attenuation effect to be small. As I demonstrated, the degree of attenuation in bivariate models is proportional to the degree of associations between the concepts and their indicators. In addition, we must assume *no* model specification error. (That is, the model is theoretically correct.)

Another reason for arguing that unmeasured theoretical concepts should be made explicit in social science theories, in addition to the attenuation effect, is that indicators may change over time and space. For example, in Figure 1, X' (field army affiliation) is a *time-space specific* indicator for the concept of informal associations among members of the PLA elite. It (X') will have no effect on PLA coalitional behavior after the last pair of PLA elite members who once had served in the same field army are dead. However, factional politics in the PLA then may *shift* to another empirical referent, or indicator, such as to coalitions of elite members who once served in the same military region.

Thus, indicators are spatially and temporally specific, and unless their theoretical concepts are stated clearly, there will be no criteria for changing indicators. We may then conclude wrongly that causal relationships between theoretical variables have changed when, in reality, only the indicators, or their relationships with the theoretical concepts, have changed.[6]

From Verbal to Mathematical Formulations

Blalock states that inventory of causes models are the most likely results of theory construction efforts during the initial phase of theoretical development in a field (Blalock, 1969). Such models specify all independent variables as exogenous variables that cause the behavior of one dependent, endogenous variable.

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[4] If we rewrite $r_{X'Y'}$ as $(r_{XX'}r_{YY'})r_{XY}$, then it is evident that the degree of attenuation in $r_{X'Y'}$ is dependent proportionally on the sizes of $r_{XX'}$ and $r_{YY'}$.

[5] If either $r_{XX'}$ or $r_{YY'}$ is zero, then $r_{X'Y'}$ equals a quantity (either $r_{XY}r_{XX'}$ or $r_{XY}r_{YY'}$) multiply by zero, which, of course, is zero.

[6] After the theoretical concepts are specified, causal relationships among concepts can be estimated if multiple indicators can be obtained for *each* concept. This estimation procedure is the multiple indicators technique. The practical problem is that multiple indicators which will satisfy the conditions for applying this technique are hard to find in Chinese politics.

A careful analysis of Whitson's work suggests that he has in mind an inventory of causes model of PLA coalitional behavior. His model is often treated as bivariate because the current debate centers around one of his independent variable, field army affiliation. What is often neglected is that Whitson (1973) has suggested three other variables: the PLA elite member's commander or commissar role, generation orientation, and geographic origin.

Three points need to be raised about these three variables. First, Whitson does not specify whether they are to be considered as additional independent variables of PLA coalitional behavior. I shall assume that they are, since Whitson writes about competing PLA elite coalitions formed *according* to the elite member's generation, geographic origin, or ideological orientation. Second, it is not clear whether these independent variables, including the variable of field army affiliation, are truly exogenous variables. Whitson does state that these variables are related. However, since no *causal relationships* among these variables are *specified,* I shall take the liberty to assume that they are exogenous and to rewrite the Whitson model as an inventory of causes model. Third, the new variables are indicators and not theoretical concepts. They are, in fact, background variables which are especially prone to *measurement errors.* Quoting Blalock (1964, p. 164): "Background variables, such as region, are complex indicators that are related in unknown ways to a given underlying (unmeasured) variable." Like region, "generation" and "commander or commissar role" are indicators of complex underlying theoretical concepts.

The underlying concept of the indicator "geographic origin" is regional loyalty. The hypothesis is that members of the PLA elite tend to recruit, promote and defend those actual or potential elite persons who are also from the same region. Whitson suggests that PLA elite members can be classified as either "southern elite" members—those from the provinces of Hunan, Hupeh, Szechuan, or Kiangsi—or "northern elite" members—those from the other provinces. Southerners are, in general, older than northerners, and they also hold higher positions in the PLA. Southerners try to exclude northerners from the top positions, which results in the competing northern and southern coalitions in the PLA.

One can see the undesirable result of using background variables as indicators. Take geographic origin as an example. Its effect on PLA coalitional behavior is confounded by the influences of at least two other variables: age and a "bottleneck effect" (or frustration of upward mobility). If we were to obtain a path coefficient for geographic origin on coalitional behavior, we will not be sure whether that coefficient is due to the effect of region, age, thwarted upward mobility, or to some other variable. Technically, we may run into complicating problems of undetected interactions (more on this later). The net result is that the model will be extremely unreliable in its findings and conclusions.

Unfortunately, Chinese studies is a data-weak field for well-known intellectual and political reasons that need not be discussed here. Therefore, in Chinese studies, one is often forced to use rather unreliable background variables. However, "data-weak" is a relative term and can be applied to all areas of social sciences. For those in especially weak areas, such as Chinese studies, the theoretical *assumptions* and empirical *effects* of using background variables must be spelled out explicitly. One should, therefore, bring as many variables into a model as possible in order to control for all possible confounding effects, and then proceed to test such a model with weak data. For this reason, the variables age and bottleneck effect will be brought into the last mathematical model of this article.

The variable generation indicates the year in which a PLA elite member first joined the Chinese Red Army. Whitson's hypothesis (1973) is that PLA elite members are more likely to form coalitions with those from the same generation. He points out that each succeeding generation of PLA elite personnel, in general, has become more and more professional. Professionalism is defined by Whitson as the reliance on organization and fire power rather than on political purity. Whitson identifies eleven generations of PLA elite personnel (1969, 1973).

Again, there are confounding factors involved here. Are these coalitions formed because of generational ties, or because of differences in the degree of professionalism? And, therefore, is a generational coalition a form of interest group or a type of faction? What are the conceptual differences between professionalism and "commander versus commissar role"—the other exogenous variable in the model? If there is none, then we will face a technical problem called multicollinearity.[7]

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[7] Multicollinearity occurs when two independent variables are highly correlated. If a condition of multicollinearity exists, then one can no longer esti-

What is the causal relationship between region and generation in the PLA? As I argue elsewhere (Ting, 1976), southerners generally were recruited first and, therefore, region is causally antecedent to generation. Since these "exogenous variables" are related *causally*, then the Whitson model, as reformulated, is theoretically misspecified.

The third indicator of coalitional formation in the PLA, according to Whitson (1973), is "commander versus commissar role." Historically, commanders and commissars have resisted the sharing of power with each other. Commanders want to keep their troops "professional," but the commissars want to stress the political purity of the troop. Ideally, the dialectic nature of leadership leads to the creation of an army that is both "red and expert."

The underlying theoretical concept here is political ideology. The commanders and commissars are presented by Whitson as *ideal types*, the two ends of a continuum ranging from the pure ideologue to the pure technocrat. How ever, in reality, most PLA commanders and commissars probably fall somewhere in between. One of the best-known commissars, Teng Hsiao-ping, for example, is also one of the best-known technocrats.

This leads to another technical problem relating to scaling. Whitson treats both variables *geographic origin* and *commander versus commissar role* as dichotomous measurements. As I argued above, there are few, if any, pure ideologues and pure technocrats. It is doubtful if the concept of regional loyalty can be measured adequately by a dichotomy of southerners versus northerners too. Technically, we may have mistakenly measured underlying *interval* scales with dichotomous, nominal scales. The effects of such "down scaling" are discussed in the literature on measurement errors (Blalock, 1964, 1969). In general, the smaller the number of categories in the variable, the larger the degree of attenuation in the correlation will be. Indeed, biased estimates may result too. Again, I can only state the problems, and proceed to use the weak indicators since it is difficult to create, for example, a Guttman scale of Chinese elite personnel according to their regional affinities or role preferences.

The variable generation is treated as an ordinal measurement by Whitson, and he identifies eleven generations of PLA elite members. It is hard to accept his argument that transitions from one generation to the next have been discrete occurrences. Rather, these transitions must have been occurring continuously. Mathematically, this means that an interval scale is a more accurate measurement. Therefore, I will identify the generation of PLA elite members by the *year* in which they entered the service of the Red Army.

To summarize, I have reformulated the Whitson model into a five-variable inventory of causes model, and I have argued that the Whitson model, as I reformulated it, is causally misspecified since some of the exogenous variables in the model are endogenous variables in reality. In addition, the causal relationships between the background variables and their theoretical concepts are not clear.

To test the Whitson model empirically, we must see how well its theoretical predictions fit empirical data. An inventory of causes model yields the following predictions (I shall use Y to denote the endogenous variables and X's to denote the exogenous variables): The first prediction is that $r_{X_iX_j} = 0$, where i and j are all possible pairs of exogenous variables in the model. This prediction is intuitively understandable, since all X's are exogenous variables. We can predict next that $| r_{X_iY.X_j} |$ is greater than $| r_{X_iY} |$.[8] Finally, since all X's cause Y directly, we can predict that $r_{X_iY.X_j \ldots X_n} \neq 0$, where j through n are the other exogenous variables in the model, except i. The Whitson model is represented mathematically in Figure 2.

The revised model as presented in Figure 2 can be tested now. The data (primary data gathered by the author) consists of 423 members of the PLA elite during the period 1972–1973, the critical purge period that followed the Lin Piao incident. Following Whitson (1973), I shall define an elite member as one holding the rank of deputy commander

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mate the separate effect of each independent variable. A case of perfect multicollinearity is when X and Z are theorized to cause Y, but $r_{XZ} = 1$. Consequently, the partial correlations or $r_{XY.Z}$ and $r_{YZ.X}$ are mathematically undetermined, since:

$$r_{xy.z} = (r_{xy} - r_{zx}r_{yz})/\sqrt{(1 - r^2_{xz})\,(1 - r^2{}_{yz})} =$$
$$(r_{xy} - r_{yz})/\,0.$$

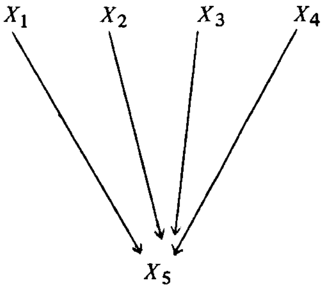
The same holds for $r_{zy.x}$.

[8] Since $r_{ij} = 0$, therefore:

$$|r_{iy.j}| = |(r_{iy} - r_{ij}\ r_{yj})/\sqrt{(1 - r^2_{ij})\,(1 - r^2_{yj})}|$$
$$= |r_{iy}/\sqrt{1 - r^2_{jy}}| \geqslant |r_{iy}|\,.$$

or deputy commissar at a military district or army corps, or higher. It is estimated that there are approximately 500 such elite members in the PLA system (Ting, 1975). Therefore my sample contains virtually the entire PLA elite population. However, there are only 100 central PLA elite members as compared to 400 regional PLA elite members. (In my sample, there are 80 central and 343 regional PLA elite members.) Since the variable central and regional affiliations is a key point of contention between Whitson and Nelsen, I have selected a stratified sample of 80 members of the central elite and 80 members of the regional elite for this study.[9] The regional elite members were selected by a random sampling technique.

Ideally, the data should be panel data, since time is important to the Whitson, Nelsen, and Parrish debate. However, an acceptable substitute for a panel data set is one covering a single point in time *after* 1967. This is because of Nelsen's argument that the field army model cannot be applied to post-1967 PLA politics. In addition, the period chosen for study must be a crisis period so that personnel changes and policy conflicts are intensified in the PLA. Thus, the bureaucratic model can also be tested by such a data set. I have chosen the post-Lin purge period because it satisfies the two conditions set above. In addition, the post-Lin purge was an important era politically, yet it has not been thoroughly studied and documented by students of Chinese politics.[10]



X_1 = Field Army (*FA*)
X_2 = Generation (*GEN*)
X_3 = Commander/Commissar (*C/C*)
X_4 = Region (*REG*)
X_5 = Purge Pattern (*PP*)*

Assumptions

1. Linear and additive
2. $C(e_i, e_j) = 0$

Predictions

1. $r_{ij} = 0$
2. $|r_{i5.j}| \geqslant |r_{i5}|$, where *i* and *j* are all possible pairs of exogenous variables.

Source: William Whitson (1973), *The Chinese High Command*, New York: Praeger.

*Coding procedures for X_1 through X_4 are discussed in the article. The purge pattern variable is coded as follows: 1 = purged; 2 = demoted; 3 = remained stable; 4 = promoted; 5 = newly recruited.

Figure 2. The Whitson Model

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[9] An argument can be made that a study of coalitional behavior among the Chinese elite in general, both civilian and military, would enhance one's power to generalize. This argument is valid; however, there are two reasons why such a comprehensive study is not done here. First, this study is related theoretically to other studies of Chinese military coalitional behavior. Therefore, for standardization and comparability, I will use the same elite population as Whitson, Nelsen, and Parrish. Second, the development of the Chinese Communist party always has been intertwined with that of the Red Army. Consequently, there always has been an overlapping of the Chinese civil and military elites. For example, the Chinese Communist party's Central Committee has a high proportion of military representatives. (See, for example, Weng, 1972, pp. 160–62.) Therefore, a data set containing the PLA elite members also contains a significant proportion of the Chinese civilian elite members. Of course the "pure" civilian elite members left out of this study may constitute a nonrandom group with respect to their coalitional behavior. See Sung (1975).

[10] Since the primary purpose of this study is the construction of an integrated general theory of coalitional behavior among PLA elite members, and since scientific knowledge is accumulative, the explanation of the Lin Piao crisis is left to other sources. A good source is Kau (1975).

Table 1 shows the goodness of fit of the Whitson model by comparing its theoretical predictions to empirical evidence.

The findings in Table 1 support my contention that the Whitson model as represented in Figure 2 is misspecified theoretically. Four of the six zero predictions have actual coefficients larger than .1, ranging from .111 (r_{14}) to .242 (r_{23}). These wrong predictions suggest that causal relationships do exist among these exogenous variables. Two of the partial correlations also turn out to be smaller than the corresponding zero-order correlations (in absolute values) suggesting that the model in Figure 2 is misspecified.

Both standardized path coefficients (p_{ij}) and unstandardized path coefficients (b_{ij}) are reported in Table 1. Standardized path coefficients are more appropriate for within-sample comparisons, and unstandardized path coefficients are more appropriate for cross-sample comparisons (Blalock, 1967 and Duncan, 1966). They are both reported because the ultimate theoretical objective here is to create a general theory of coalitional behavior, but our testing is limited to one sample.

According to the findings in Table 1, we can infer that the most important cause of PLA coalitional behavior *in the Whitson model* is generation ($p_{52} = -.339$). This supports Whitson's hypothesis that older PLA elite members have been purged in order to make room at the top for younger ones. The most interesting finding is that field army affiliation is only the third most important cause of PLA coalitional behavior, after generation and region.

These findings support Nelsen's and Parrish's contention that the field army affiliation was no longer important to PLA politics after 1967. Of course, this does not mean that *factional politics* were no longer important for the PLA elite during the post-Lin purge, since the two most important causes of coalitional behavior in Table 1 (generation and region) are indicators of factional politics. The amount of variance in the variable coalitional behavior "explained" by the four independent variables is .195, or 19.5 percent.[11]

The Nelsen Model. The Nelsen model suggests, in causal model terms, a multiplicative model, that is, one containing interactive functions. A multiplicative first-order interaction states that a given phenomenon, the dependent variable, is most likely to occur when two factors, the independent variables, are *both* present (Blalock, 1969). Therefore, the dependent variable

Table 1. Testing the Whitson Model

Zero-Predictions	Actual	Prediction: $\lvert r_{x_ix_5.x_j\ldots x_k}\rvert \geqslant \lvert r_{x_ix_5}\rvert$	
		Partial *r*	**Zero-Order *r***
1. $r_{12} = 0$	−.185	1. $r_{15.234} = .187$	$r_{15} = .240$
2. $r_{13} = 0$	−.010	2. $r_{25.134} = -.323$	$r_{25} = -.159$
3. $r_{14} = 0$	.111	3. $r_{35.124} = -.045$	$r_{35} = .121$
4. $r_{23} = 0$	.242	4. $r_{45.123} = -.300$	$r_{45} = -.262$
5. $r_{24} = 0$	.116		
6. $r_{34} = 0$	.017		

Ordinary Least Squares Estimates of Structural Parameters for *X*5

Independent Variables	b_{ij}	p_{ij}	Significant at the .05 Level
*X*1 (*FA*)	.192	.190	No
*X*2 (*GEN*)	−.160	−.339	Yes*
*X*3 (*CC*)	−.183	−.041	No
*X*4 (*REG*)	−.815	.282	Yes
$r = .442$		$R^2 = .195$	

Source: Primary sources are *People's Daily* (Jin-min jih-pao), *Peking Review, Red Flag* (Hung-chi), and *Central Daily News* (Chung-yang jih-pao), 1972–1973. Secondary sources are United States Department of Defense (1973), *Chinese Military Personnel,* Washington, D.C.; Central Intelligence Agency, *Military Organization of the People's Republic of China,* Washington, D.C., DOCEX Project, 1972. Biographical sources are Huang Chen-hsia (1968), *Mao's Generals,* Hong Kong: Research Institute of Contemporary History; William Whitson (1973), *The Chinese High Command,* New York: Praeger.

*Significant at the .01 level.

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[11] Although one speaks of the amount of variance "explained," one has actually established a statistical association with the coefficient R^2.

will be small if *either* independent variable approaches zero.

Nelsen argues that the field army affiliation was not an important factor in PLA politics after 1967. This argument can be presented as a first-order interaction between two independent variables, field army affiliation and *time*, which jointly affect the coalitional behavior of PLA elite members. The variable time has a positive value before 1967, but has a zero value after 1967. Since the product of any positive value with zero is zero, then this first-order interaction, in effect, states that field army affiliation has no impact on PLA coalitional behavior after 1967.

Nelsen gives no reason for the demise of the field armies after 1967. Was it due to the diminishing field army loyalty through time? Was it due to the high degree of dissonance that existed between the central and regional PLA elite members, created by the 1967 central directive to support the revolutionary left? The answer lies beyond the scope of this paper, but it is reasonable to assume that the death of the field army cliques (if they indeed died) was a continuous rather than a discrete event. Since the height of the field army days probably occurred in 1947 and the "death" in 1967, we can assign the value of 1 to 1947, decreasing incrementally to zero in 1967 (and after). This would represent a continuous and linear trend.

Nelsen also argues that professional interests, as represented by the indicator of central-regional interests, has been the most important independent variable of PLA coalitional behavior since 1967. Applying the same logic, we can treat central-regional interests and time as the two independent variables of a first-order interaction which jointly affect PLA coalitional behavior. In this interaction, the years 1967 and before are assigned the value of zero and the years after 1967 are assigned the value of one.

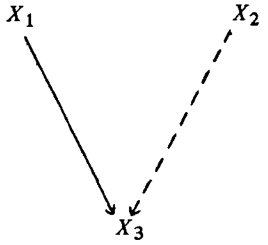
Panel data are necessary in order to test the Nelsen model. Unfortunately, our data contain information only on a single period: 1972–1973. However, we can reject the Nelsen model empirically if the partial correlation of field army with coalitional behavior, control for central-regional interests, is non-zero. The Nelsen model is presented in Figure 3.

The Nelsen model can be tested empirically now. Table 2 below shows the goodness of fit of the Nelsen model by comparing its theoretical predictions with empirical evidence.

The single zero-prediction deduced from the Nelsen model, $r_{23.1} = 0$, turns out to be incorrect. This would suggest that there may be a *direct* effect from X_2 (*FA*) to X_3 (*CB*). The path coefficient p_{31} (the effect of central-regional interests on coalitional behavior) for 1972–1973 was –.579, indicating that a central elite member was more likely to be purged than a regional elite member during the post-Lin purge. The amount of variance explained in Table 2 is .110, or 11 percent.

A Nonrecursive, Simultaneous-Equations, and Multiplicative Causal Model of PLA Coalitional Behavior

Theories contain both axioms and theorems. In social science theories, statements that state direct causations are axioms and covariance statements derived from the axioms are the-



Zero-Prediction

$r_{23.1} = 0$

X_1 = Central-Regional Conflict (*CR*)*
X_2 = Field Army (*FA*)
X_3 = Purge Pattern

Assumptions

1. Linear
2. Additive
3. $C(e_i, e_j) = 0$

Source: Harvey Nelsen (1972), "Military Forces in Chinese Military Politics," *China Quarterly* 51:444–74.

*Central-Regional Conflict is coded as follows: 1 = a central elite member; 0 = a regional elite member.

Figure 3. The Nelsen Model

orems (Blalock, 1969). Therefore, axioms are causal laws, and these laws are tested indirectly with theorems.

The Axioms. My nonrecursive model of PLA coalitional behavior is based on five theoretical axioms, from which theorems are derived. The axioms are stated below:

AXIOM 1. Scarcity of goods and values causes coalitions to form in a political system.

A question prior to the study of coalitional behavior in general should be: "Why are coalitions formed in the first place?" Coalitions are formed in order to protect existing, and to obtain additional, goods and values desired by the members of the coalitions. This suggests that a condition of scarcity of some goods and/or values, desired by two or more competing coalitions, must exist as a precondition for coalitions to form in a political system.

AXIOM 2. The structures of coalitions and the style of coalitional behavior in a political system are caused by the degree of political legitimization and the level of institutional autonomy granted by that political system to the coalitions.

If the legitimacy and the structural independence of political coalitions are guaranteed by laws or by political norms of a political system, such as the United States political system, then coalitions in such a system can be *structurally independent* from the government and are likely to make *formal* demands upon the government. On the other hand, if structurally independent coalitions are not sanctioned by laws or norms, such as the Chinese political system, then political coalitions, by necessity, are not likely to have formal structures or to make formal demands. Hence, the competing coalitions and the issues involved in the former type of political system are easier to identify than in the latter type of political system, since the structures and the formal demands of the former are clearly defined.

AXIOM 3. Coalitions can be formed on the basis of professional interests, and/or *on the basis of affective ties among the members.*

Another way of asking *how* coalitions are formed is to ask what *types* of goods and values are worthy of being contested by competing political coalitions. In general, the goods and values can be classified into two categories—professional interests and affective ties with one's friends and/or ethnic group.

AXIOM 4. The goal of a coalition is to compete favorably for the goods and values in question.

If coalitions are formed to adapt to scarcity, then the goal of any coalition must be that of obtaining the goods and values in question. A winning coalition must be able to do three things: it must deny the same goods and values to competing coalitions, obtain more of the goods and values in question whenever possible, and protect the coalition's existing goods and values.

AXIOM 5. Coalitional behavior has feedback functions.

The outcome of coalitional competition at time *T* will influence the distribution of scarcity at time *T*+1. Since scarcity is given, therefore some coalitions must be deprived of certain desired goods and values at time *T*. Thus, a scarcity condition may be perpetuated into time *T*+1. This new distribution pattern may, in turn, (a) dissolve some of the ineffective

Table 2. Testing the Nelsen Model

Zero-Prediction	Actual
$r_{23.1} = 0$	.127

Ordinary Least Squares Estimates of Structural Parameters

Independent Variables	b_{ij}	p_{ij}	Significant at the .05 Level
$X2$ (*FA*)	$b_{32} = .389$	$p_{32} = .144$	No
$X1$ (*CR*)	$b_{31} = -1.206$	$p_{31} = -.579$	Yes
$r_{13} = .332$		$R^2_{13} = .110$	

Source: Primary sources are *People's Daily* (Jen-min jih-pao), *Peking Review, Red Flag* (Hung-chi), and *Central Daily News* (Chung-yang jih-pao), 1972–1973. Secondary sources are United States Department of Defense (1973), *Chinese Military Personnel,* Washington, D.C.; Central Intelligence Agency, *Military Organization of the People's Republic of China,* Washington, D.C., DOCEX Project, 1972. Biographical sources are Huang Chen-hsia (1968), *Mao's Generals,* Hong Kong: Research Institute of Contemporary History; William Whitson (1973), *The Chinese High Command,* New York: Praeger.

coalitions in the system that have been consistent "losers," (b) force some coalitions to combine forces for collective security (merger of interests), (c) force other coalitions to mutate by changing their strategies and/or goals at time $T+1$, and (d) create new coalitions at time $T+1$.

The Theorems. The nonrecursive model of PLA coalitional behavior is structured as follows. First, following Axiom 3, I separated the *causes* of PLA coalitional behavior into two main categories—factional politics and interest group politics. This allows one to test the bureaucratic model. Also, from a theoretical point of view, it is important to determine the relative importance of factions versus interest groups in Chinese political behavior; this is a key question in the Whitson, Nelsen, and Parrish debate. Second, two new exogenous variables are brought into the model—age and upward mobility. The reason for adding these variables was discussed earlier, and need not be repeated here. Third, following Axiom 5, I have added a feedback function to the Ting model.

The third point warrants further technical discussions. The Whitson and Nelsen models, as I reformulate them, are *recursive* models (Blalock, 1964; Wonnacott and Wonnacott, 1970). That is, they assume that a clear causal hierarchy can be established among the variables in their models. A relatively simple technique, the ordinary least squares technique, can be used to yield unbiased estimates of the parameters in a recursive model. Furthermore, among linear-additive estimators, the ordinary least squares technique has the smallest variance ($v(b)$ = minimum) (Johnston, 1972; Rao and Miller, 1971; Theil, 1969). Thus, recursive models have their attractions, but the *assumptions* of the ordinary least squares technique must not be violated. Specifically, the causal relationships must be (a) linear, (b) additive, (c) without nonrandom measurement errors, and (d) $c(e_i e_j) = 0$ where c is covariance (Blalock, 1975).

If reciprocal causations exist in a model, then *d* will be violated and ordinary least squares will provide biased estimates of the parameters ($E(b) \neq B$) (Blalock, 1969). In the PLA case, it is perhaps unrealistic to assume that the style of coalitional formation affects the outcome of coalitional behavior, but that the outcome does not affect the style in return. Rather, the relationship is probably reciprocal rather than recursive. The question is, is the two-way causation instantaneous or delayed? If a lag period is involved, then the difference equation method—essentially ordinary least squares with time series data—can be used to provide unbiased estimates *if* there is no autocorrelation ($c(e_t, e_{t-1}) = 0$). If no lag period is involved, then a simultaneous equations technique, such as the two-stage least squares technique, should be used in order to obtain unbiased estimates *if* the equation is at least exactly identified (Blalock, 1975; Johnston, 1972; Rao and Miller, 1971; Theil, 1969). Since I do not have time series data on PLA coalitional behavior, and since there are feedback functions in my nonrecursive model, the two-way causation in the model must be treated as instantaneous, and two-stage least squares can be used as the estimation technique.

This brings up another technical issue: underidentification, which means that there are too many unknowns in a particular equation to yield a unique estimate for each parameter in the equation. Therefore, *exogenous* variables that cause variation in some—but not all—of the endogenous variables involved in the reciprocal causation must be added to that model. In general, if there are K number of endogenous variables in the equation, then at least $K - 1$ number of variables in the model must be *left out* of the equation for it to be identified (Blalock, 1975).

A practical problem in constructing technically advanced, nonrecursive theories in social sciences is that such exogenous variables are very hard to locate and measure. Thus, if I hypothesize that the outcome of coalitional behavior and the upward mobility of PLA elite members are reciprocally related, then two exogenous variables that satisfy the condition set in the above paragraph for identification must be located and be brought into the model. This is what I hypothesize in my model. Therefore, I used the elite members' rankings in 1949 (the establishment of the People's Republic of China) and in 1973 as the two exogenous variables needed to identify the equations involved. Hypothetically, the ranking of a member of the PLA elite in 1949 affects how he or she views his own rate of upward mobility today, but does not affect how such a person will behave politically. The reverse is true for an elite individual's ranking in 1973. These exogenous variables are far from ideal, but one must identify and measure the most effective exogenous variables that are *available*.

Multiplicative, or nonadditive, functions may exist in a model. It is logical to assume, for example, that a coalition can form among members of the PLA elite who come from the same region *and* have the same professional interests (Axiom 3). Technically, this is called a first-order interaction. Four sets of such interactions are hypothesized in my model (see

Figure 4b). My nonrecursive model of PLA coalitional behavior is presented in Figures 4a and 4b. The indicators for factional politics are on the top halves of the models, and the indicators for interest group politics are on the bottom halves.

Figure 4a shows an additive model with one set of reciprocal causation between X_6 and X_7 (upward mobility and coalitional behavior). Thus, the model can be treated as a recursive model from X_1 to X_5, and ordinary least squares can be used to estimate the parameters up to that point (X_5). The addition of exogenous variables Z_1 and Z_2 (rankings in 1949 and 1973) makes equations X_6 and X_7 over identified. A third exogenous variable Z_3 (age) is added to Figure 4a for reasons I discussed earlier. Since equations X_6 and X_7 are identified, a simultaneous equations technique, the two-stage least squares technique, can be used now to obtain unbiased estimates ($E(b) = B$) of their true parameters.[12]

Figure 4b should be superimposed onto Figure 4a. They are presented individually because a single, combined model would contain too many causal arrows, making the model hard to comprehend. More importantly, I separated them because Figure 4b contains only the multiplicative aspects of the Ting model. Figure 4b has four sets of first-order interactions. As I stated earlier, a first-order interaction exists when a given phenomenon (coalitional behavior) is likely to occur when two independent variables are both present. Thus, for example, the interaction between region and central-regional interests on coalitional behavior tests the theoretical likelihood of coalitions forming among PLA elite members who are from the same region *and* who share professional views.

My nonrecursive model is tested in Table 3. All the zero-predictions are tested. However, due to the limitation of space, only the results of equation X_7 (coalitional behavior) are presented for comparative purposes. In any case, the correctness of the zero-predictions should indicate whether the model *as a whole* is correctly specified. Since equation X_7 is one of the two equations involved in a set of reciprocal causation, the simultaneous equations technique of two-stage least squares is used to estimate the parameters.

Of the eight partials predicted to be zero, six actually have coefficients of less than .1. The two incorrect predictions both involve one set of bivariate, additive relationship: p_{64} (commander-commissar role to upward mobility). The incorrect partials are .152 and .123. Therefore, Figure 4a should be revised by adding a causal arrow from X_4 to X_6. That is, the PLA elite members' commander or commissar orientation has a direct effect on their rates of upward mobility from 1949 to 1973. Specifically, commanders have been promoted faster than commissars during this period, since the values for partial coefficients $r_{46.12357}$ and $r_{46.137}$ are positive.

Substantively, the findings in Table 3 indicate that the two most important causes of coalitional behavior among members of the PLA elite in 1973, by far, were central-regional interests (X_5) and the first-order interaction between the variables generation and central-regional interests, with respective path coefficients of –.873 and .832. Thus, the findings of my model support the hypothesis that members of the central elite were much more likely to be removed during the post-Lin purge, but, interestingly enough, that they were more likely to be *protected* if they were both central elite members *and* from one of the four southern provinces. This may indicate a curious mixture of factional *and* interest group politics in the Chinese political system, a system where coalitions are not structurally autonomous from the government (Axiom 2). (I will have more on this in the concluding section.) The next in order of importance as a cause of coalitional behavior among PLA elite individuals was another interaction between two factional referents, region and generation (p = –.78) followed next by the variable region (p = .44).

What can we conclude about the current field army debate? According to the nonrecursive model—which is the only model thus far that has not been rejected empirically[13]—field army affiliation was not an important cause of PLA coalitional behavior in 1973 (p = .381). On the other hand, one cannot conclude that the field army variable has no effect at all, since p_{73} is non-zero. One can, however, conclude that central-regional interests were much more important than field army loyalty in determin-

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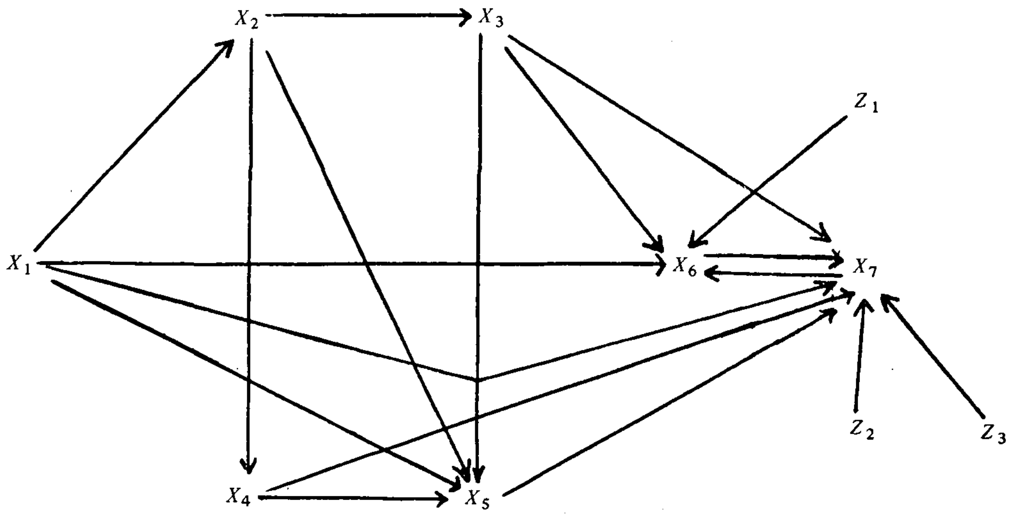
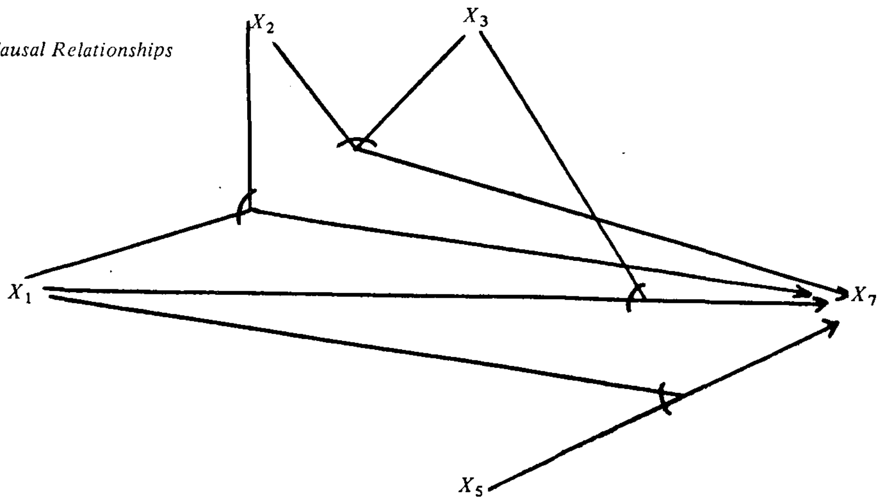
[12] Note that the two-stage least squares technique is asymtotically unbiased in this case, and the estimators obtained have larger variance than ordinary least-squares—a biased estimation technique in this case (Rao and Miller, 1971, p. 214). Therefore, unless the sample size is "large," the two-stage least squares technique does not have an advantage over the ordinary least squares technique. By any definition, our sample size can be considered a "large" sample.

[13] One cannot establish the *absolute* correctness of a theory or model in any scientific discipline.

4a. Nonrecursive and Additive Causal Relationships



4b. Multiplicative Causal Relationships



Indicators for Factional Politics (Endogenous Indicators)

Regional Affiliation	(X_1)
Generational Tie	(X_2)
Field Army Affiliation	(X_3)
Rate of Upward Mobility[a]	(X_6)

Indicators for Interest Group Politics (Endogenous Indicators)

Commander/Commissar Role	(X_4)
Central/Regional Interest	(X_5)

Exogenous Variables

Ranking in 1949	(Z_1)
Ranking in 1973	(Z_2)
Age	(Z_3)

The Indicator for Coalitional Behavior[b]

Purge Pattern	(X_7)

Zero-Predictions

$r_{13.2} = 0$ $r_{34.12} = 0$
$r_{14.23} = 0$ $r_{34.2} = 0$
$r_{14.2} = 0$ $r_{46.12357} = 0$
$r_{26.1345} = 0$ $r_{46.137} = 0$

Source: Axiom 1 through Axiom 5 of this article.

[a] The rate of upward mobility is measured as: $X_6 = Z_2 - Z_1$.

[b] Coalitional behavior in a closed political system can only be inferred from observable behavior. The assumption here is that the purge pattern of the PLA elite members during a time of crisis reflects underlying coalitional behavior of contending groups and factions in the PLA. This assumption appears to be valid since one can often distinguish purges from cases of "normal retirement."

Figure 4. A Nonrecursive Causal Model of PLA Coalitional Behavior

ing PLA coalitional behavior during 1972–1973.

Was factional politics or interest group politics the dominant style of coalitional formation in the PLA during the period studied? The answer to this question will indicate whether the bureaucratic model is valid or not. According to the bureaucratic model, factional politics should dominate during a period of crisis. Since the period studied was a time of crisis, we would expect those indicators on the top halves of Figures 4a and 4b to be higher than those at the bottom halves. Ideally, all path coefficients of the indicators at the bottom halves should be zeroes.

The findings do not provide a clear-cut answer here. Central-regional interests are the most important causes of X_7 (coalitional behavior) in Table 3, but the first-order interaction between a factional indicator (generation) and an interest group indicator (central-regional interests) is almost as important. To add to the complexity, the smallest path coefficient is p_{74} (commander-commissar role to coalitional behavior); commander-commissar role (X_4) is an indicator *for interest group politics.*[14] We can, however, conclude that the PLA coalitional behavior during 1972–1973 was not *dominated* by factional politics, and that probably both types of coalitions were operating simultaneously.[15]

Conclusion

Before the Cultural Revolution in China (1966–1969), scholars viewed members of the Chinese elite as a cohesive group relatively free from policy competition. Therefore, concepts and theories of pluralistic elite analysis were not applicable to the analysis of Chinese elite behavior.

Table 3. Testing the Nonrecursive Model

Zero-Predictions	Actual	Zero-Predictions	Actual
1. $r_{13.2} = 0$	.094	5. $r_{34.12} = 0$	–.014
2. $r_{14.23} = 0$	–.012	6. $r_{34.2} = 0$	–.023
3. $r_{14.2} = 0$	–.012	7. $r_{46.12357} = 0$	.152
4. $r_{26.1345} = 0$	–.020	8. $r_{46.137} = 0$	.123

Two-Stage Least Squares Estimates of Structural Parameters for Equation *X*7

Independent Variable	$\hat{\hat{b}}_{ij}$	$\hat{\hat{p}}_{ij}$	Significant at the .05 Level
*X*1 (*REG*)	5.302	.437	Yes
*X*3 (*FA*)	.486	.381	Yes
*X*4 (*CC*)	.045	.007	No
*X*5 (*CR*)	–14.084	–.873	Yes*
*X*6 (*UM*)	–.012	–.023	No
*W*1 (*GEN* & *CR*)	.318	.832	Yes*
*W*2 (*REG* & *GEN*)	–.151	–.780	No
*W*3 (*REG* & *CR*)	–.140	–.039	No
*W*4 (*REG* & *FA*)	.251	.282	No
*Z*2 (*Rank* '73)	–.301	–.171	No
*Z*3 (*AGE*)	.089	.251	Yes

$r = .656$ $R^2 = .430$

Source: Primary sources are *People's Daily* (Jen-min jih-pao), *Peking Review, Red Flag* (Hung-chi), and *Central Daily News* (Chung-yang jih-pao), 1972–1973. Secondary sources are United States Department of Defense (1973), *Chinese Military Personnel,* Washington, D.C.; Central Intelligence Agency, *Military Organization of the People's Republic of China,* Washington, D.C., DOCEX Project, 1972. Biographical sources are Huang Chen-hsia (1968), *Mao's Generals,* Hong Kong: Research Institute of Contemporary History; William Whitson (1973), *The Chinese High Command,* New York: Praeger.

*Significant at the .01 level.

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[14] The commander-commissar role conflict is a weak indicator for the unmeasured concept of political orientation or ideology. Also, p_{74} is not significant at the .05 level.

[15] Since no comparable analysis of PLA coalitional politics for normal periods is available, one cannot generalize to such time periods.

The Cultural Revolution has shifted the analysis of Chinese elite behavior from a monolithic approach to a pluralistic one. Members of the Chinese elite, as events showed, also have been competing for favorable allocation of goods and services. Therefore, such pluralistic concepts as coalitional behavior and interest groups, when modified to fit the Chinese context, can be used to analyze Chinese elite behavior. Similarly, the concept of factionalism, usually used in the Chinese context, may in turn be useful for analyzing coalitional behavior in parliamentary states.[16]

However, as this article has demonstrated, there are at least two structural and stylistic differences between Chinese coalition behavior and Western coalition behavior. First, since formal interest groups (those structurally independent from the government and with formal memberships) are not allowed in communist states, it is actually easier—and more likely—for cross-factional interest groups to form (more on this later). On the other hand, and for the same reason, it is easier to identify interest groups and their demands in parliamentary states than in communist states.

What are the strengths and weaknesses of the causal model approach to the analysis of a pluralistic elite in data-weak cases? Its strengths lie in its ability to force one to clarify theoretical concepts and to define them operationally, to formalize verbal issues into testable hypotheses, and to allow the systematic rejection or modification of a theory. Its obvious weakness is that data-weak cases are especially prone to measurement errors. Where data gathering is difficult, as in most communist states, one is forced to use crude indicators, background variables, and poor measurements. More attention should be paid to measurement errors in quantitative Chinese studies, and in communist studies in general. One result of measurement errors, as this paper has demonstrated, is that correlation coefficients may be attenuated, yielding deflated results. This is not an indictment of causal analysis per se; it is an indication of how poor we are when it comes to theory and data.

We can conclude the following: first, as for the Whitson-Nelsen-Parrish debate, we found that central-regional conflicts were more important in explaining PLA coalitional behavior than field army affiliations during the period studied. However, this should not lead to the conclusion that factional politics is dying, or that professional interests will soon dominate PLA politics, since field army is a time-space specific *indicator* of factional politics. Other indicators of factional politics in the PLA, such as regional loyalty (a Whitson variable), were still important during the Lin crisis. In addition, we may have overlooked other stronger indicators of PLA factionalism.

Second, PLA politics, contrary to the predictions of the bureaucratic model, were not oriented toward factional politics during the Lin crisis. With the exception of regional loyalty among PLA elite members, all the other indicators of factional politics in Table 3 have weak b_{ij}'s and p_{ij}'s. However, one cannot generalize from this one case study to all crises in the PLA. Also, weak indicators of factional politics may have been used, thereby attenuating the relevant regression coefficients and path coefficients.

Third, the strong influence of cross-factional interest groups during the post-Lin period showed an important point. It is crucial to point out that these groups are neither traditional interest groups nor traditional factional groups. They are formed among elite members who share both professional interests and affective ties. As I argued, this type of elite coalitions may be more common in political systems where coalitional group memberships and group boundaries are not clearly defined (Axiom 2). In political systems where group membership is institutionalized, it may be more difficult for such cross-factional interest groups to form.

There is a pressing need for scholars to devote even more time to the study of coalitional behavior and elite competition within communist states. Political events in China since 1966 have demonstrated the inadequacies of monolithic models (Townsend, 1974). A parallel development has taken place in Russian studies (Skilling and Griffith, 1971). That scientists are primarily interested in the validity of theories may be noble, but the practical consequences of using wrong theories of communist coalitional behavior can have disastrous results.

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[16] I am very grateful to my colleague Allen Whiting for showing me this possibility.

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Intergovernmental Organizations and Foreign Policy Behavior: Some Empirical Findings*

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In this study, we evaluate whether the increase in the number of intergovernmental organizations (IGOs) has resulted in their increased use for foreign policy behavior by the nations of the world. This question is examined in three related ways: (1) the aggregate use of IGOs for foreign policy behavior; (2) the relationship between IGO membership and IGO use; and (3) the kinds of states that use IGOs. Our data base consists of the 35 nations in the CREON (Comparative Research on the Events of Nations) data set for the years 1959–1968.

The main findings are that IGOs were employed over 60 percent of the time with little fluctuation on a year-by-year basis, that global and "high politics" IGOs were used more often than regional and "low politics" IGOs, that institutional membership and IGO use were generally inversely related, and that the attributes of the states had limited utility in accounting for the use of intergovernmental organizations. Some of the theoretical implications of these findings are then explored.

The growth of intergovernmental organizations (IGOs) has been astounding since the end of World War II. Although only about 100 IGOs existed in 1950, the number of such organizations has reached well over 250 by the mid-1970s (Wallace and Singer, 1970, pp. 272, 275–80; Kegley and Rochester, 1971, pp. 403–04; and the *Yearbook of International Organizations,* 1974, 1977). Moreover, this growth has occurred not only in terms of geographical distribution of such organizations but also in terms of functional issues with which they deal. Despite a considerable literature documenting this growth in the number and types of IGOs, only a scant amount of research relates this institutional growth to their use for coordinating foreign relations among nations.[1] Thus we know little about the extent to which nations use IGOs as a foreign policy forum, the kinds of nations that use them, and what impact such organizations have on the conduct of foreign policy behavior.

In this study, we address some of these concerns by investigating empirically the linkage between foreign policy behavior and the use of intergovernmental organizations. Specifically, we examine two interrelated sets of questions. The first set of questions focuses upon the *aggregate use of IGOs:* (1) To what extent do nations employ IGOs for conducting foreign policy? (2) To what extent do nations use the various types of IGOs (e.g., global versus regional and "high politics" versus "low politics") for this behavior? (3) Have these patterns changed over time? A second set of questions examines the *characteristics of nations that act or interact with one another through IGOs.* In particular, we focus upon the relationship between IGO membership and IGO use and between the national attributes of states and IGO use. The questions addressed here are: (1) Is IGO membership related to the use of these organizations? That is, do nations that hold memberships in a large number of IGOs also tend to employ them more frequently than those without such extensive memberships? Similarly, does this pattern hold when we examine the relationship between membership and the use of various types of organizations (global, regional, high politics, and low politics)? (2) To what extent are specific national attributes (such as the relative size of the country and the level of economic development) related to IGO use by nations? Do small, developing states, for example, utilize IGOs more extensively than large, developed states?

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[1] For a review of the empirically based research on international organizations, see Alger (1970). Also, see Kihl (1971) for a previous study of IGO-nation-state linkage.

What differences exist between states in their use of the different types of IGOs?

Three important concepts frame our subsequent discussion: *IGOs, IGO use,* and *foreign policy behavior.* While we discuss foreign policy behavior below, let us now define IGOs and IGO use. The term intergovernmental organizations (IGOs) refers to those organizations which have nations as members in contrast to nongovernmental organizations (NGOs) which have individuals and private groups as members. The specific definitional criteria used to identify IGOs followed the standards set down by the United Nations and the *Yearbook of International Organizations:* "All organizations established by agreements to which three States or more are parties. . . ." Using these criteria, the IGOs included here ranged from such well-known organizations as the United Nations and the North Atlantic Treaty Organization to such lesser-known organizations as the Cocoa Producers Alliance and the International Commission for the Protection of the Rhine. IGO use refers to *any* activity by a nation in which an intergovernmental organization is engaged as a forum for fostering national policy, as an instrument of that policy, or as an arena for coordinating behavior with other states.[2] Some illustrations of foreign policy behavior within IGOs (taken from the data set discussed below) might be helpful in clarifying our definition. Activities within IGOs ranged, for example, from the United States giving assurances to their NATO allies against external threats, to Kenya, at a meeting of the Organization of African Unity, accepting an offer from Zambia to mediate its border dispute with Somalia to Venezuela asking the Organization of American States to take action against Cuban aggression.

The Data

The evidence with which to answer our research questions was drawn from the data set compiled by Charles Hermann and his associates, called Comparative Research on the Events of Nations (CREON). It is supplemented with a separate data collection on IGO membership for the nations in the sample. We chose the CREON data set because it was coded in a way which indicated whether a nation's foreign policy behavior (or event) occurred within the context of an international organization or outside of it.[3] When the behavior occurred within an international organization, the name of the specific IGO was coded so that we knew the exact context in which the foreign policy action took place.

The CREON data consist of 11,665 foreign policy events initiated by some 35 nations gathered during randomly selected quarters (January-March for 1961, 1965; April-June for 1960, 1963, 1967; July-September for 1964, 1966; and October-December for 1959, 1962, 1968) to represent the 1959–1968 decade. Although the rationale for the selection of the CREON nations, the definition of what constitutes a foreign policy event, and a description of the extensive precautions in collecting and coding these events are fully reported elsewhere (Hermann et al., 1973, esp. pp. 15–31), we need to identify three potential data problems in using the CREON data set and to offer some caveats about making inferences from it.

The first potential problem is that only a sample of nations is used in the analysis. While the set of states in the CREON sample is obviously not wholly representative of the nations in the international system, nonetheless, a cursory inspection of the CREON list in the appendix makes clear that many of the principal types of states have been included. For example, the CREON nations not only represent a wide variation in physical size, political orientation, and economic development, but also include all geographical areas of the world.

The second problem is that the CREON data are taken from a single source, *Deadline Data on World Affairs.* This necessarily raises the question of "source coverage"—a problem endemic to all events data sets. A number of studies on this topic have noted that a single source is particularly troublesome because of the low degree of reporting overlap between sources and because of the tendency of some newspapers and chronologies to report only certain kinds of "newsworthy" events (Azar et al., 1972; Doran et al., 1973; Smith, 1969; and Scolnick, 1974). In support of their data coverage, the CREON investigators point out that they used the *uncollapsed Deadline Data* files for their coding procedures (rather than the briefer collapsed set usually available) and that, in their on-site inspection of the *Deadline Data* operation, summaries of the world press were widely used. As they note (Hermann et al., 1973, p. 18): "Events for our 35 nations are derived from 46 sources of wide geographical, functional, and ideological variation ranging, for example, from the New York *Times* to Havana Radio to the *International Financial News Survey* to *L'Unité Africaine*—all of which are abstracted by *Deadline Data.*" Despite this coverage, they nevertheless do call for attempts to isolate the extent of bias in their data.

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[2] For a recent informative discussion identifying these uses of international organizations, see Butterworth (1978, pp. 7–8).

[3] The specific variable from the CREON data set that was used to measure behavior within an IGO was variable 9. In a few instances (less than 4 percent of the total), events occurred within the CREON list of international organizations that would not qualify as IGOs according to our definition (e.g., "countries generally associated with the U.S.," "nonaligned countries," etc.). Since these behaviors constitute such a small portion of the total, we included them with the IGO behavior category for analytic purposes. Each of these events, of course, was subsequently classified according to the nearest type of IGO as outlined below. See the organizational appendix in the CREON Project Codebook (Hermann et al., 1975, p. 29) for further examples of these instances.

The third potential source of bias is the use of yearly quarters to represent years. This is a possible problem because some parts of years may be more active in international politics than others (Hermann et al., 1973, p. 25). Thus, some yearly totals may be greater than others owing solely to the portion of the year sampled. Although Hermann et al. acknowledge some differences in their summary description of the data, the differences that they report between quarters (and thus years) are quite small. They range from a low of about 23 percent of the activity in the first quarter to a high of over 26 percent in the third quarter (pp. 24–25). For this reason, the problem would appear to be negligible in altering the results of our analysis.

But this assessment of the data fails to take into account foreign policy behavior occurring *only* within IGOs. Would this type of behavior be under-represented or over-represented owing to the quarter that was sampled? The potential problem is that some IGOs tend to have greater activity within particular quarters (principally the last quarter of the year) and thus would more likely be reported and subsequently collected in the CREON data set. According to our breakdown of the yearly data, the percentage of IGO behavior within each quarter ranges from 6.1 percent for 1960, when the April-June quarter is used as the sampling quarter, to 14.1 percent for 1962 when the October-December quarter is used. However, the other quarters are within a much narrower range, from 9.3 to 11.6 percent for the other eight years. Judging from these results, it seems that undersampling or oversampling of certain IGO behavior is not a major constraint upon our investigation. This conclusion, however, cannot be hard and fast for at least two reasons. First, there is no assured way of determining the universe of all IGO behavior, dependent as we are upon the coverage of the data source. Second, differences between quarters (and thus years) also reflect real differences in the use of IGOs for foreign policy activities; therefore, quarterly differences would be expected.

When we further analyzed the types of IGO behavior reported in the quarters, we did find different levels of "low politics" and "regional" IGO behavior as compared to "high politics" and "global" IGO behavior. Again, however, the exact source of these differences cannot be attributed solely to underreporting but undoubtedly reflect real differences in the level of use by nations. On balance, then, our position is that particular kinds of IGO activity may be somewhat under-represented—owing to the potential source coverage problem and to the use of quarters as yearly representatives—but that real differences are also reflected in the data. Overall, we do not believe that this potential source of error will alter the thrust of our general findings because the differences between the various IGOs turn out to be quite substantial.

In sum, while we recognize these three possible data biases (and thus caution against pushing the results too far), we nonetheless concur with the CREON investigators that their data set does offer a good first approximation of the foreign policy behavior of the 35 nations sampled. Additionally, and crucially for the study at hand, it is the only data set to our knowledge that offers a readily available means of identifying such behavior in the context of IGOs.

The other part of our data, consisting of information on organizational memberships for the 35 CREON nations, was taken from the *Yearbook of International Organizations* (1969).[4] By examining the membership lists of the 219 IGOs in the *Yearbook*, we were able to ascertain the extent to which each of the CREON nations belonged to them.[5] Next we coded each of the IGOs according to geographical and functional type.[6] These two categories were chosen because a more exacting breakdown would seriously reduce the number of IGOs in each group and would therefore make any analysis more difficult and less useful. More importantly, these two categories have direct relevance for our subsequent evaluation of the regionalist and functionalist arguments. We also distinguished between those organizations that were either global or regional in membership. Such an organizational distinction was usually readily apparent when we surveyed the membership lists. The only difficulty arose over those organizations that had member-states derived primarily from one region but which also included at least one non-regional member-state—the so-called "quasi-regional" organizations (Nye, 1971, p. 8). At this point, we made a research decision to include these organizations within the regional category because there were only a limited number of these organizations and because we could more easily test the regionalist argument.

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[4] The 1969 edition of the *Yearbook* was chosen because it appeared toward the end of the CREON data years (1959–1968) and would likely incorporate most of the IGOs that had been in existence throughout the period. On occasion, other *Yearbooks* were consulted to ascertain accurate membership information.

[5] In the appendix, we note the number of IGOs to which each of the CREON nations belonged. Overall, a total of 221 distinct IGOs were identified in the *Yearbook*. Of those, two IGOs (International Diplo-

Finally, the functional classification also divided the intergovernmental organizations into two groups: each IGO was classified as either a "high politics" or a "low politics" organization. The classification scheme was based upon Pentland's discussion (1976, pp. 628–29) of four types of international organizations: (1) diplomatic-military, (2) economic management or development, (3) technical-functional, and (4) social and cultural. The first category formed our "high politics" organizations group and the latter three formed the "low politics" organizations group. To classify the IGOs, we categorized each of the organizations into the "high politics" and "low politics" groups using the Pentland types as a guide. The title and our knowledge of the IGOs were usually sufficient for this task. However, this classificatory task was made easier by consulting the various functional breakdowns (some 28 in all) employed in the 1974 *Yearbook*. These served as valuable guides to the identification of the main activities of each organization. Despite these 28 categories, though, it was sometimes necessary to go to a description of the activities of the IGOs within the *Yearbook* in order to classify them. Additionally, when an IGO seemed to engage in both high and low politics activities, we had to make a judgment regarding its primary activity. This occurred only in rare instances.

Findings

Extent of IGO Use by the CREON Nations. The first part of Table 1 shows the extent to which IGOs have been used by the CREON states as a forum for conducting foreign policy. According to these results, 66.1 percent of foreign policy actions (calculated by averaging the percentages of use by each nation) were taken within the context of IGOs for the period 1959–1968.[7] For the yearly breakdowns,

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matic Academy and International Children's Centre) were excluded from the analysis. For the former, it was not possible to locate an accurate membership list despite numerous attempts. For the latter, the organization did not prove to be one consisting of individual nations as members.

[6] The behavior data occurring within IGOs (and extracted from CREON variable 9) were subsequently collapsed into global, regional, high politics, and low politics IGO behavior following essentially the same procedure discussed for the IGO membership data.

[7] These percentages were calculated in the following way: (1) the foreign policy events (or behavior) for each nation were divided into those that occurred within and outside IGOs; (2) percentages of behavior within each category were then calculated for each nation; and (3) mean percentages for both categories were computed (only the percentages of behavior within IGOs are portrayed in Table 1 for each year and in total). The figures in parentheses in the first part of Table 1 are the number of foreign policy events contributed by all nations for the particular year and in total. We calculated the rest of the percentages in Table 1 and all the percentages in Tables 3 and 4 using a similar format.

It is important to emphasize that the percentages shown are the mean percentages of behavior which we calculated by averaging the mean percentages for each individual nation. We believe this measure is more meaningful than simply calculating the mean level of foreign policy events across a year or in total, as has been done in past research. This latter approach fails to take into account the *relative* contribution of each nation. For example, active states with many foreign policy actions in one category could disproportionately skew the results when events are the unit of analysis. This is less likely to be the case when the nation is the unit of analysis.

At this juncture, we should emphasize that we have not attempted to measure the importance of the kinds of foreign policy behavior undertaken within or outside IGOs; rather, our concern is the *use* of these organizations for foreign policy purposes. Nations obviously do not use IGOs for matters of equal importance; some nations use IGOs to state their political beliefs about the world, others request collective security action by an organization. Comparisons based upon the frequency of such use have their limitations; however, we hope to extend the research later to assess types of IGO use. Until that is moreover, IGO use still remained over 50 percent for every year. What is interesting to note is the lack of great fluctuation in the use of IGOs even in a period marked by a rise in the number of such organizations. Thus it appears that IGO use was not seriously affected by the growth of such organizations during this decade.

Focusing only on the behavior within IGOs, we next determine whether there have been any differences in the pattern of use between geographical and functional IGOs for foreign policy purposes (also see Table 1).[8] For the geographical dimension, regional IGOs had considerably less importance than did global IGOs as the forum for foreign policy during the 1960s. In the aggregate, regional organizations

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completed, though, our present analysis can begin to reveal some useful information about IGO use in international politics.

[8] Some illustrations of the kinds of foreign policy behavior that took place within regional/global and low politics/high politics IGOs may be helpful at this point. For instance, activities ranged from Belgium declaring in the United Nations Security Council that it is categorically opposed to the secession of Katanga Province, to France opposing the 25 percent general quota increase in the International Monetary Fund, to Kenya, Tanzania, and Uganda signing a preferential trade agreement with the European Common Market to the Ivory Coast signing the International Coffee Agreement sponsored by the United Nations.

Table 1. Foreign Policy Behavior and IGO Use of the CREON Nations by Context and Types of IGOs

	By Context[a]			By Types of IGOs[b]			
	Percent Within IGOs			Percent Within Regional IGOs	Percent Within Low Politics IGOs		
1959	80.5[c]	N=32	(876)[d]	27.1	34.6	N=31	(626)[e]
1960	59.5	N=32	(846)	44.5	21.0	N=31	(346)
1961	70.9	N=32	(1083)	31.4	18.1	N=32	(605)
1962	70.6	N=33	(1567)	29.3	25.8	N=33	(888)
1963	63.8	N=33	(1357)	36.8	18.5	N=31	(726)
1964	68.2	N=34	(1305)	25.3	73.2	N=33	(731)
1965	56.9	N=35	(1035)	25.8	47.7	N=34	(386)
1966	67.7	N=35	(1140)	18.2	68.6	N=34	(711)
1967	51.6	N=35	(1397)	35.3	36.5	N=34	(590)
1968	70.9	N=35	(1059)	14.4	28.0	N=34	(659)
Total	66.1[f]	N=336	(11,665)	28.6	37.6	N=327	(6318)

Source: CREON (Comparative Research on the Events of Nations) data set, 1959–1968.

[a] Context in Tables 1 and 3 refers to foreign policy behavior occurring within or outside IGOs.

[b] For all tables, regional IGOs are organizations whose memberships are restricted to a particular geographical area while global IGOs are organizations whose memberships are open to all states. Low politics IGOs are those which deal with issues of economic, technical, and social-cultural affairs while high politics IGOs are those which deal with issues of political and military affairs. The text describes the procedures used in coding these organizations.

[c] Entries represent the mean percentages of behavior within each category. These were computed by averaging the mean percentages for each nation in a particular year (or in total). The percentages shown subtracted from 100 give the percentage of foreign policy behavior outside IGOs, within global IGOs, and within high politics IGOs, respectively.

[d] The Ns represent the number of CREON nations that acted within and/or outside IGOs for each year. (The N for the total category is the sum of the Ns for the individual years.) The figures in parentheses are the number of foreign policy events contributed by all nations in the particular year or in total.

[e] The Ns represent the number of CREON nations that acted within regional and/or global IGOs as well as within low politics and/or high politics IGOs. (They are the same for these different types.) The N for the total category is the sum of the Ns for the individual years. The figures in parentheses are the number of foreign policy events contributed by all nations in the particular year or in total.

[f] Difference of means tests were calculated for within/outside, regional/global and low politics/high politics percentages. All were significant beyond the .001 level.

were not employed more frequently than global organizations. The average use of these regional IGOs was under 29 percent, while the average use of global IGOs was about 71 percent. For the functional dimension, high politics IGOs were employed most frequently; these organizations were used about 62 percent of the time while the low politics organizations were used less than 38 percent of the time. Nonetheless, low politics organizations were used extensively in three particular years—1964, 1965, and 1966—but the pattern of such use was not sustained across the other seven years. On the whole, then, we must emphasize that the low politics IGOs were used infrequently during the 1960s.

These sets of results seem to call into question some of the regionalist and functionalist arguments regarding international organizations. First of all, our findings for the 1959–1968 period imply that regional organizations have not gained much currency as foreign policy arenas for the CREON nations. Thus our results do not seem very encouraging for the theoretical arguments which suggest that *regional* organizations may be important means of creating "islands of peace" in the international system (Nye, 1971; Yalem, 1965; and Falk and Mendlovitz, 1973). Despite the rapid development of regional IGOs, the use of such organizations for foreign policy apparently has not accelerated at a similar pace. In fact, a plot of the regional IGO data over time shows a small but consistent downturn in use.

The data in Table 1 also seem not to support the functionalist position (Mitrany, 1943; Haas, 1964; Sewell, 1966; Claude, 1971, pp. 378–407; and Pentland, 1973, pp. 64–146). According to this view, low politics concerns are less controversial than high politics concerns and are thus more likely to foster the process of multilateral cooperation. If this argument holds, we should see the use of IGOs expand in low politics areas and eventually into the high politics areas, too. Our aggregate trend data for the low politics organizations, however, do not seem to support such a view; rather, we detect a slight downturn over the years, despite the brief 1964–1966 upswing. This result is somewhat surprising when one recalls that the low politics organizations have increased more rapidly than any other type of intergovernmental organization (Pentland, 1976, p. 628). Since our inferences are based upon an analysis of a particular time span, both of these conclusions regarding the regionalist and functionalist arguments must remain tentative. We shall, however, have more to say about each conclusion as we analyze the linkage between IGO membership and IGO use, and the characteristics of nations that employ IGOs.

IGO Membership and IGO Use. Table 2 shows correlations between IGO membership data for each nation (total membership, global membership, regional membership, high politics membership, low politics membership, and the various combination of these) with the appropriate foreign policy use by each state within these types of organizations.[9] The first important finding is that total membership and total use do produce a significant correlation, albeit a negative one (–.36). Because it is negative, the correlation implies that the more IGOs a nation belongs to, the less it uses these organizations for foreign policy purposes. Such a result suggests that *increased membership ties have not necessarily produced increased IGO use; rather, these organizations have been utilized much more selectively.*

There are some exceptions to this conclusion which qualify what we said earlier about the regionalist and functionalist arguments. As also indicated in Table 2, regional IGO membership and regional IGO use show a significant positive correlation of .39, while global membership and global IGO use produce an insignificant negative correlation of –.25. The implication of the first statistic is that regional ties have had a positive impact on nations using such IGOs: *the more a nation joins regional IGOs, the more likely it is to use them for foreign policy behavior.* A parallel statement cannot be made for global organizations. (In fact, the global data suggest an insignificant inverse relationship.) In this sense, those who argue for a regionalist approach to solving international problems can find some solace in these results. Thus, although global IGOs are most widely used, the degree of correspondence between joining IGOs and actually using them appears more evident in regional IGOs.

The other significant results from Table 2 are the correlations for the high politics and low politics organizations. Membership in high politics IGOs is inversely related to their use (–.64), while membership in low politics IGOs is positively correlated with use in these organizations (.47). What this first correlation seems to suggest is that the nations with few such memberships use them extensively. For example, CREON nations like Ivory Coast, Guinea, Ghana, and Egypt, among others, held few memberships in high politics organizations, yet a large portion of their foreign policy behavior occurred within these organizations. Conversely, CREON nations like France, West Germany, Japan, and Italy, among others, which belonged to many high politics organizations used them less extensively for their IGO foreign policy activities. Both of these patterns within the high politics organizations contribute to the negative correlation that we obtained. By way of contrast, the percentage of IGO behavior within the low politics organizations roughly corresponded to the extent of memberships in these organizations by the CREON nations. *The more memberships in these low politics organizations, the greater the use of such organizations for foreign policy.*

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[9] The exact procedure for obtaining these correlations was as follows. We first calculated membership "scores" for the CREON nations in each of the membership categories (global, regional, etc.). Next we computed the percentage of IGO behavior for each nation within each similar organizational category. Finally, we correlated the appropriate pair with one another using the Pearson product-moment correlation coefficient.

Such results suggest that a closer linkage exists between memberships in low politics organizations and using them for foreign policy purposes than for high politics organizations. In this limited sense, the functionalist argument seems to garner some support. Moreover, this position is enhanced even more if one considers that the only other significant correlation in Table 2 is for regional low politics (.51). But again, while such trends are encouraging to the functionalist position, they must be tempered by the fact that the high politics organizations are most extensively used (Table 1).

National Attributes and IGO Use. The first set of figures in Table 3 shows the relative use of IGOs (as compared to other means of conducting foreign policy) controlling for the size of the nation, the level of economic development, and a combination of these two factors. Evidently, small states have used IGOs to a much greater extent than large states; the degree of difference is quite substantial—73 percent as compared to 52 percent. When we compare this difference between large and small states with what we obtained for the development dimension or for a combined size and development dimension (although this one reaches statistical significance), size still appears to be the more discriminating factor in accounting for relative IGO use. Moreover, this finding suggests that small states see IGOs as a particularly good way to carry on foreign policy at a relatively low cost (East, 1973, p. 565).

When we examine behavior within IGOs, however, the relative size of the nation does not seem to be the key factor that accounts for differences in use of the geographical and functional IGOs (also see Table 3). In fact, for the global/regional IGO breakdown, none of the national attribute characteristics produces significant differences in the relative use of these organizations. Although all states use global IGOs more extensively than regional IGOs, the differences between types of states are always small and statistically insignificant. Even when we combine these two dimensions (section C of Table 3), we do not find larger differences between the states. Thus our analysis by type of state does not differentiate among patterns of use within geographical types of organizations. For the functional breakdown, on the other hand, the results are somewhat more encouraging. While neither size nor a combination of size and development produces statistically significant differences, the level of economic development does. Developing states use high politics IGOs significantly

Table 2. Intercorrelations between IGO Membership and IGO Use by the CREON Nations, 1959–1968

Total Membership and Total Use	(N=35)	–.36*
Global Membership and Global Use	(N=34)	–.25
Regional Membership and Regional Use	(N=34)	.39*
High Politics Membership and High Politics Use	(N=34)	–.64**
Low Politics Membership and Low Politics Use	(N=35)	.47**
Global High Politics Membership and Global High Politics Use	(N=33)	–.06
Global Low Politics Membership and Global Low Politics Use	(N=33)	.01
Regional High Politics Membership and Regional High Politics Use	(N=28)	–.18
Regional Low Politics Membership and Regional Low Politics Use	(N=28)	.51**

Source: CREON (Comparative Research on the Events of Nations) data set 1959–1968 and *Yearbook of International Organizations*, 12th ed. (1969). Brussels: Union of International Associations.

Note: Entries are Pearson *r*s. N=12

*Significant beyond .05.
**Significant beyond .01.

more than developed states, while the latter use low politics IGOs significantly more than developing states.

What this analysis implies is that poor states, irrespective of size, tend to employ IGOs that will gain them the most political and economic benefits. These are more often the global and high politics organizations. Such results are generally consistent with some previous findings on small/large state behavior, although we find that the developing/developed dimension is more potent. A previous study (East, 1973, p. 576), for instance, discovered that small states tend to act more selectively in conducting foreign policy than do large states and tend to seek maximum impact with their actions. A similar pattern appears to occur for the developing states in our data; they tend to be selective in their employment of IGOs and seek the greatest payoff when they use them—primarily through the global and high politics IGOs. Finally, this set of results highlights the fact that developed states are apparently the only kinds of nations that have a sufficient resource base and a large enough foreign policy bureaucracy to use global, regional, and low politics organizations simultaneously.

We can obtain an even clearer picture of the relative utility of these national attributes in differentiating among levels of IGO use by one final analysis. Table 4 presents levels of IGO use by the four types of states with a more refined categorization of IGOs (global high politics, global low politics, regional high politics, and regional low politics). All states use global high politics organizations to a much greater extent than any other type of IGO. Usage ranges from 41 percent for large developed states to 59 percent for large developing states. But these differences prove to be statistically insignificant by our analysis of variance test; therefore, we must conclude that, even here, the type of state does not have any substantial impact on the use of global high politics organizations. A similar

Table 3. Foreign Policy Behavior and IGO Use of the CREON Nations by Context and Types of IGOs, Controlling for Size and Development[a]

	By Context		By Types of IGOs		
	Percent Within IGOs		Percent Within Regional IGOs	Percent Within Low Politics IGOs	
A. By Size					
Small States	73.3*[b]	N=23 (4709)[c]	29.2	35.2	N=23 (3353)[d]
Large States	51.6	N=12 (6956)	30.8	37.1	N=12 (2965)
B. By Development					
Developing States	68.6	N=15 (3498)	27.7	27.9*	N=15 (2102)
Developed States	63.8	N=20 (8167)	31.3	41.8	N=20 (4216)
C. By Size and Development					
Small Developing	77.2*	N=12 (2241)	27.5	30.8	N=12 (1490)
Small Developed	69.1	N=11 (2468)	30.1	41.7	N=11 (1863)
Large Developing	34.2	N= 3 (1257)	25.2	22.8	N= 3 (392)
Large Developed	57.4	N= 9 (5699)	32.7	41.9	N= 9 (2573)

Source: CREON (Comparative Research on the Events of Nations) data set, 1959–1968.

[a] For a discussion of how the CREON nations were classified by size and development and a listing of nations in each category for Tables 3 and 4, see the appendix.

[b] Entries represent the mean percentages of behavior within each category. These were computed by averaging the mean percentages for each nation. The percentages shown subtracted from 100 give the percentage of foreign policy behavior outside IGOs, within global IGOs, and within high politics IGOs, respectively.

[c] The Ns represent the number of CREON nations that acted within and/or outside IGOs by type of state. The figures in parentheses are the number of foreign policy events contributed by each type of nation.

[d] The Ns represent the number of CREON nations that acted within regional and/or global IGOs as well as within low politics and/or high politics IGOs. (They are the same for these different types.) The figures in parentheses are the number of foreign policy events contributed by each type of nation.

*Significant differences beyond the .05 level for difference of means test and one-way analysis of variance. Because there was not homogeneity of variances for the low politics analysis of variance, the Kruskal-Wallis test was employed. The result was not significant.

conclusion results for the global low politics analysis. All types of CREON nations use these organizations at about the same rate, except for the large developing states. This difference is undoubtedly attributable to the fact that China failed to use these organizations at all during the ten years of our data base. The regional high politics and low politics organizations, however, exhibit some differences; small developing states use the former most frequently, and the developed states use the latter most often. But here, too, we cannot say much more about these differences since all fail to reach conventional levels of statistical significance. Thus this more refined analysis by the four types of IGOs fails to improve our earlier limited results on national attributes and IGO use.

On balance, then, national attributes do not seem to delineate IGO use very effectively. Instead, we must conclude that the level of IGO use is related less to the type of state and more to the type of intergovernmental organization under consideration.

Conclusion

These findings on the use of IGOs for foreign policy behavior have some important substantive and theoretical implications. First of all, while the aggregate use of IGOs for foreign policy has been quite substantial over the decade of our analysis (averaging over 60 percent), traditional interstate procedures for conducting foreign affairs have remained relatively stable. This is evidenced by the lack of an increase in the level of IGO use over time, despite a growth in the number of these organizations. Thus, the enlargement of the network of IGOs does not appear to have affected appreciably the existing pattern of nation-state relations. The actual pattern of IGO use seems consistent with the realist's description of the limited role of intergovernmental organizations: IGOs are used by nations primarily as selective instruments for gaining foreign policy objectives.

A second finding buttresses this conclusion regarding the realist argument. Our data also show that increased institutional ties in world politics have generally *not* been translated into increased use of IGOs—at least for the years 1959 to 1968. Contrary to our initial expectations, institutional growth and usage have been inversely related. In this sense, while formal ties between states have expanded in recent years, the degree of IGO use for foreign policy has not increased correspondingly. For the regional and low politics IGOs, however, this observation is less applicable. In these instances, increased membership *is* tied to increased use. In a limited way, then, the regionalist and functionalist arguments have some validity for enhancing the level of multilateral activities and for constructing a web of interdependence among sovereign states. Put differently, the functional agencies have had some impact on altering the prominence of bilateral diplomacy for carrying on interstate relations. However, we must emphasize the limited nature of this functional strategy of international cooperation in light of our overall results.

A third major finding—the prominent use of high politics organizations by all types of

Table 4. IGO Use by the CREON Nations for Four Types of IGOs and States (Percent)

	Type of IGO				
	Global High	Global Low	Regional High	Regional Low	
Small Developing	43.3[a]	29.2	25.9	1.6	N=12 (1490)[b]
Small Developed	42.3	27.5	16.0	14.1	N=11 (1863)
Large Developing	58.7	16.1	18.5	6.7	N= 3 (392)
Large Developed	40.7[c]	26.8	16.8	15.7	N= 9 (2573)

Source: CREON (Comparative Research on the Events of Nations) data set, 1959–1968.

[a] Entries represent the mean percentages of behavior within each category. These were computed by averaging the mean percentages for each nation in a given category. Totals do not always add to 100% due to rounding error.

[b] The Ns represented the number of CREON nations that acted within the various IGOs. The figures in parentheses are the number of foreign policy events contributed by each type of nation.

[c] One-way analysis of variance (by type of state) was calculated for the four types of IGOs. All results were insignificant. Because there was not homogeneity of variances for the regional low politics organizations, the Kruskal-Wallis test was employed. The result was not significant.

states—further demonstrates the limitation of this functional perspective. The high politics organizations—political and military ones—dominate the level of use even in a period when the low politics IGOs—the economic, technical, and social-cultural ones—grew more rapidly. Thus the success of the functional approach to international institution-building seems mainly confined to the low politics arena and has not appreciably affected the high politics ones. Also the fact that IGO membership in these organizations does not correlate positively with IGO use implies that the use of high politics IGOs is a selective one, apparently keyed to achieving specific foreign policy objectives. Our last finding—the tendency for developing states to use global and high politics organizations more extensively than regional and low politics organizations—also emphasizes the basic utilitarian nature of IGO use. Because the poor states cannot use all types of IGOs (constrained as they are by domestic resource requirements), they tend to select the ones most likely to gain them the maximum benefit.

The constraints of the CREON data set do not allow us to go further and make more definitive statements about the total impact of IGOs in altering foreign policy behavior. Nevertheless, the thrust of our results points to one important conclusion: despite the growth in the number of IGOs during the 1960s, the usage of these organizations for foreign policy purposes remained unchanged. Whether this pattern holds for the 1970s is certainly open to question, especially in light of a changing international system, increasingly characterized as "complex" and "interdependent" (Keohane and Nye, 1977). Our evidence for the 1960s,

Appendix. List of Total IGO Memberships and Foreign Policy Actions for the CREON Nations, Controlling for Size and Development

Small Developing			Small Developed		
Costa Rica	174[a]	(57)[b]	Belgium	385	(105)
Ghana	272	(48)	Chile	204	(64)
Guinea	215	(29)	Cuba	325	(51)
Ivory Coast	180	(56)	East Germany	165	(5)
Kenya	97	(49)	Iceland	177	(48)
Lebanon	160	(43)	Israel	321	(56)
Philippines	214	(47)	New Zealand	211	(55)
Thailand	162	(51)	Norway	234	(75)
Tunisia	277	(58)	Switzerland	72	(71)
Uganda	154	(50)	Uruguay	150	(51)
Yugoslavia	256	(56)	Venezuela	224	(65)
Zambia	80	(34)			
Large Developing			Large Developed		
China	455	(30)	Canada	394	(70)
India	490	(71)	France	849	(118)
Turkey	312	(66)	Italy	431	(100)
			Japan	273	(71)
			Mexico	190	(75)
			Spain	173	(74)
			USSR	965	(49)
			USA	1880	(87)
			West Germany	544	(95)

Source: The classification by size and development is drawn from Philip M. Burgess (1970), "Nation-typing for Foreign Policy Analysis: A Partitioning Procedure for Constructing Typologies," in Edwin H. Fedder (ed.), *Methodological Concerns in International Studies,* St. Louis: Center for International Studies, pp. 3–66. Table 4 from Appendix A and Appendix B were used because they provided the "best" partitions. The cut-off point for dividing between large and small states was a population greater than and less than 23.7 million, and for dividing between developed and developing states was a GNP per capita greater than and less than $401. For the "misclassifications" even with these partitions (for Cuba in the development category; for Canada, Philippines, and Thailand in the size category), the composite results were employed to classify these states. See pp. 19–21 for this discussion. For another use of the Burgess scheme in attribute analysis, see Maurice A. East (1973), "Size and Foreign Policy Behavior: A Test of Two Models," *World Politics,* 25: 556–76.

[a] Number of foreign policy events initiated by each nation, calculated from the CREON (Comparative Research on the Events of Nations) data set, 1959–1968.

[b] Number of IGOs to which each nation belonged, calculated from the 1969 *Yearbook of International Organizations,* 12th ed., Brussels: Union of International Associations.

however, should serve as a caution against drawing premature conclusions.

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A Swedish Road to Socialism: Ernst Wigforss and the Ideological Foundations of Swedish Social Democracy*

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The reputation of Ernst Wigforss (1881–1977), the foremost ideologist of Swedish Social Democracy, has suffered from the lack of a coherent treatment of his work in English. This article briefly surveys Wigforss' historical contributions to Swedish Social Democracy. Then it examines the structure of Wigforss' thought, analyzing first the conceptions of equality, liberty, democracy, security, economic efficiency, and solidarity that serve for Wigforss as the aims of Social Democracy. Then it considers the reformist measures Wigforss espoused to achieve these aims–social welfare policy, progressive taxation, economic planning, industrial democracy, and socialization of industry. It concludes with an assessment of Wigforss' importance as a Social Democratic theorist and suggests the relevance of his ideas to American politics and social science.

Swedish Social Democracy is commonly regarded as having a distinctive character, but not an explicitly socialist ideology or program. Indeed the reformist achievements and political dominance of the world's most successful Social Democratic party have often been traced directly to its "pragmatic" and post-ideological stance. Marquis Childs, whose influential endorsement of Sweden as a model of the welfare state's "middle way" still attracts attention, claimed that

> the wisdom of the Swedes lies above all in their willingness to compromise, to meet what appears to be reality. They have not been "bound" by a "system," nor have they been committed to dogma. In a sense they are the ultimate pragmatists interested only in the social order. This may explain why their contribution to political and social thought has been slight (1947, p. 161).

Herbert Tingsten, in his classic study of *The Swedish Social Democrats: Their Ideological Development,* argued that the Social Democrats had benefited from casting off their traditional ideological baggage and assuming a less theoretical stance (Tingsten, 1941, 1963). Gunnar Adler-Karlsson has commented on the lack of "any accepted *theory* of the middle-way economy in Sweden"; the Social Democratic constructors of the welfare state, he writes, have acted only *"as if"* they adhered to such a theory (Adler-Karlsson, 1970, pp. 3, 2). Richard Tomasson (1969, p. 773), in assessing "The Extraordinary Success of the Swedish Social Democrats," states that "in only one major way has the party failed to be outstanding; it has not contributed significantly to the development of socialist theory."

This common assumption that the Swedish Social Democrats lack a coherent ideological foundation is wrong. The inaccuracy of the conventional wisdom can be demonstrated in several ways. One can observe that it is impossible to be purely pragmatic and nonideological in politics. Explicitly or not, political figures always act with reference to particular ends; to be "pragmatic" is ordinarily to accept prevailing goals without question. One can also show the persistent socialist content in Social Democratic programs and policies, the strategy of Leif Lewin's *Planhushållnings Debatten (The Debate over Economic Planning)* (1967). Here I pursue a third strategy. I examine the writings of the Swedish working-class movement's preeminent thinker, Ernst Wigforss (1881–1977), and contend that his writings merit scrutiny both for the light they shed on the theory (and practice) of Swedish Social Democracy and for their intrinsic appeal as Social Democratic doctrine.

Although the work of his fellow Social Democrats, Alva and Gunnar Myrdal, has gained international recognition, Wigforss' writings remain untranslated and his reputation confined largely to Sweden. Within Swedish Social Democracy, however, Wigforss' impact overshadows that of the Myrdals and of other party theoreticians. Former prime minister Tage Erlander (1972, p. 18), himself strongly influenced by Wigforss, wrote that

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> Ernst Wigforss developed a coherent socialist view of society. Thereby he had an enormous influence on the thinking of generations of Swedes. At the same time he was a practical politician. He saw his social analysis as an instrument for practical politics.

Gunnar Nilsson, the chairman of the national trade union movement, observed that Wigforss "combined in a unique way his position as the working class movement's leading ideologue with a successful career as a political reformer" (*Aftonbladet,* 4 January 1977, p. 8). The more conservative wing of the party stresses his emphasis on social policy, full employment, and the possibilities of cooperation with private capital. More radical members adhere to his view that the goal of social democracy is to transform bourgeois society, not by eliminating the bourgeoisie in a violent struggle, but by democratizing the power held by capitalists. Like Wigforss, they argue that such a transformation entails industrial democracy, economic planning that directs investment, and sufficient equality of property to prevent classes based on inherited wealth. Thus an investigation of Wigforss' writings can promote understanding not only of the past successes but also of the current potential of Swedish Social Democracy.

Not even Hjalmar Branting, the celebrated founder and early leader of the party, made such a sustained and important contribution to the party's strategy and tactics. First elected to parliament in 1919, Wigforss served in either the first or second chamber of the Riksdag until 1953. From 1920 to 1952 he was a member of the directorate of the Social Democratic party and from 1928 to 1952 he sat on its executive committee. He occupied the critical position of Minister of Finance in 1925–1926 and in 1932–1949; during the latter period Wigforss' ideas underlay the policies the Social Democrats devised to direct Sweden's recovery from the Great Depression, to steer the nation through the hazards of World War II, and to engineer the postwar reforms that erected the Swedish welfare state. Wigforss exerted an important influence on foreign policy on several occasions, but here the primary concern must be the theoretical contributions that left their imprint upon Swedish Social Democratic ideology and upon the Swedish welfare state itself.

Wigforss' Contribution to Social Democratic Ideology

Wigforss helped resolve three major ideological issues that have faced the Swedish Social Democrats in their relatively short existence. First, he rejected the deterministic Marxism that commanded a substantial following within the early party and argued for a gradual transition to socialism. In the process he devised the notion of "provisional utopias" and bequeathed to the movement a style of thought which integrated long-term socialist goals, current policy proposals, and the most immediate needs of Swedish citizens. Second, he elaborated many of the economic and social policies that formed the basis of the Social Democrats' tactical program and which they carried out after they came to office in 1932. Third, after retiring from office, Wigforss refused to be satisfied with the accomplishments of the welfare state and argued that a socialist society must display greater equality and democracy. This line of argument eventually found endorsement in the 1969 party document, *Towards Equality;* in the 1970s in the legislation for industrial democracy; and in the recent Meidner proposal for wage-earner funds (Myrdal, 1971; Meidner, 1975).

In *Materialistisk historieuppfattning (The Materialist Conception of History,* 1908) Wigforss assessed the relevance of the Marxist legacy for Swedish politics (Wigforss, 1970; Lindblom, 1977, pp. 35–85). He approved of Marx's emphasis on political power and valued his analysis of the evolution of economic and social forces, but rejected the economic determinism then prevalent in interpretations of Marx. Economic and social circumstances limited, but did not ineluctably determine the course of human history, and certainly they did not guarantee the automatic emergence of socialism. In any case, Wigforss continued, in an argument reminiscent of Bernstein's *Evolutionary Socialism,* Swedish capitalism was not evolving in a way likely to generate proletarian revolution. The working class was not growing steadily poorer; on the contrary, its standard of living was rising rapidly as the trade unions combated the most brutal tendencies of capitalism. Wigforss (1964, p. 152) summarized his essay as

> on the one hand, an attempt to show what overwhelming importance economic conditions have, now only for humanity's material life but also for its spiritual life, and on the other hand, a contribution to an understanding that idealistic considerations could not be forgotten in development, that they played a necessary role, and that it was essential to give them their proper place, both to understand what they could and what they could not accomplish. It was a weighing of how a more important role for ideas might be linked in a wholly natural

> way with reduced faith in "economic necessity," which supposedly led through "pauperization" and "catastrophe" to a socialist society.

Capitalism created only the preconditions for a socialist transformation of society that Social Democrats must carry out in accordance with their ideals.

Marxism, for all its merits, was neither a science nor a valid ethical system. One could not determine that the future would necessarily have a particular character; the best that could be done was to establish probable developments. Nor could one derive goals and norms from historical developments, not even from "necessary" developments: necessity did not entail moral worth. "Socialism can never be proven," Wigforss concluded, "it is an ideal that has to be implemented" (1970, p. 173; cf. 1952, p. 141). Idealism, rather than physical necessity or psychological misery, could and should become the motive force behind the socialist transformation of society. This idealism could build upon immediate economic interests, and then "with an enlightened popular majority inspired by cultural ideals, we shall not need to wait for an economic 'catastrophe,' or a fully completed concentration and socialization of production, in order to take power and begin the work of reorganizing society" (Wigforss, 1970, p. 92).

Wigforss was a revisionist, but not a revisionist who had struck a truce with capitalist society:

> The revisionism that can be read out of my account is partly a critique of what can be called a belief in economic determinism (*en ekonomisk ödestro*), partly the conviction that a raising of the working classes' material and cultural standard will not become a hindrance but rather an underpinning for a socialist transformation of society. If there is so little mention of "growing into (*inväxande*) the future society" without a radical change of the capitalist foundation for property rights, it is because that idea is explicitly rejected (1970, p. 8).

Marx's emphasis upon the political importance of private property was appropriate; eventually control, if not necessarily formal ownership, of industrial production would have to pass from private to public hands. Wigforss affirmed Swedish Social Democracy's objective as transforming capitalist society into a classless society, but urged that it be attained through gradual and non-revolutionary means.

By detaching the construction of socialism from economic determinism, Wigforss and Swedish Social Democracy created a lack of coherence between their traditional ideology and their current, essentially liberal policy. They had replaced inevitable developmental processes with socialist ideals; they now had to apply these ideals to evolving policy situations and emerging human interests. The tension between permanent ideals and changing political interests and situations ruled out dogmatic statements about policy measures; while firm in its objectives, Social Democratic policy would have to be pragmatic and experimental in character. The device Wigforss used to link principles with policy and, in the words of Tage Erlander (1972, p. 19), to "find his and the party's way" was the *"provisional utopia."*

A provisional utopia is a tentative sketch of a desirable future society. It serves as a critique of existing social conditions and as a guide to present action, yet it can be revised in accordance with future experience. Elsewhere Wigforss, following John Dewey, called it a socialist "working hypothesis" (Wigforss, 1926; Lindblom, 1977, pp. 85–101). It presumes that the future is neither ineluctably determined nor wholly random. By tying day-to-day political work to longer-term objectives, it seeks to avoid the dangers of bogging down in purely "pragmatic" politics. It disavows "utopian social engineering," but without lapsing into the uncoordinated and potentially incoherent piecemeal tinkering that Karl Popper (1963) endorses. By creating a holistic view of socialist aspirations, the provisional utopia affords the possibility of foreseeing unexpected tensions among socialist aims or unanticipated consequences of actions. The provisional utopia's emphasis on a political creativity that blends enduring socialist aims with immediate reformist measures characterizes not only Wigforss' thought but also the theory and practice of Swedish Social Democracy.

The provisional utopia is a device for giving concrete content to Social Democratic objectives:

> Whether we talk about freedom or equality, security or adventure, personal worth or personal development, the concepts must be placed in concrete circumstances, in a situation that we can envision, so that the content we want to give them can be understood and can be compared with other interpretations of the same general expressions. Visions of social conditions different from those that surround us, that are here called utopias, thus must be developed with sufficient concreteness that they can stand out as conceivable alternatives to the given reality or to other models of the future (Wigforss, 1959, p. 44).

After World War I Wigforss began producing specific reform proposals that together might move Sweden towards a provisional Social Democratic utopia. His outstanding effort was the 1919 "Gothenburg Program" (1964, pp. 210–17). The national party did not adopt it immediately, as Wigforss had hoped, but his proposals anticipated future Social Democratic programs remarkably. Wigforss proposed, among other things, a recognized right to employment, shorter working hours, a statutory paid two-week vacation, higher old-age pensions, a national health program, maternity allowances, survivors' benefits, public support of housing construction, equal educational opportunity, progressive income and inheritance taxes, a capital levy, the socialization of finance and insurance, "the development of production and the organization of economic life under society's control," the promotion of economic efficiency, extension of consumer cooperatives, and a strong dose of worker co-determination in industry–a program that still has a radical ring to it. From it Wigforss drew virtually all of the initiatives he would propose later.

In the early 1920s Wigforss, inspired by English guild socialism and by syndicalism, began propagandizing for industrial democracy. In 1921 he assumed the chairmanship of the governmental commission established to study industrial democracy and wrote the central sections of its report. He urged that unions, in addition to striving for higher wages, should seek to promote the worker's freedom and independence in the workplace, but the unions showed little interest in Wigforss' proposals until almost 50 years later. As Olof Palme stated in his memorial address, Wigforss was "ahead of his time" on this issue, as on many others (*Arbetet*, 9 January 1977).

The minority Social Democratic governments of the early 1920s had little chance of implementing the Gothenburg proposals or industrial democracy, but by the late 1920s the prospects of obtaining a working parliamentary majority were good and the need to define practical priorities became compelling. In both the 1928 and the 1932 elections Wigforss developed the party's central campaign themes and defined the issues that separated the Social Democrats from the bourgeois parties. In 1928 he stressed the incompatibility of inherited wealth with Social Democratic principles; to diminish the social impact of inherited riches he urged not the traditional socialist remedy of socialization, but a rigorous progressive tax on property bequests (1941, pp. 191–257). Though the precise effect of these proposals upon the electorate remains unclear, the bourgeois parties seem to have capitalized effectively on imprecision in the proposals to frighten small property holders away from voting for the Social Democrats. The more subdued discussion of inheritance taxes in subsequent Social Democratic agitation supports this interpretation.

In 1932, however, Wigforss' efforts assisted the Social Democrats to a dramatic electoral success which allowed the construction of the welfare state to proceed. Wigforss' remarkable election pamphlet *Har vi råd att arbeta?* (roughly translated, *Can We Afford to Provide Employment?*) argued the case for increased public spending to offset the lack of private investment (1941, pp. 304–20; 1967, pp. 62–77; Lindblom, 1977, pp. 137–49). It illustrated the economics of the "multiplier" effect. It rebutted charges of waste. Its inspiration came not from "Keynesian" economics, but from Social Democratic principles: no longer would private calculations of profit and loss alone determine the level of employment and production; now the state would intervene to rectify the flaws of uncoordinated capitalist enterprise (Steiger, 1971; Wigforss, 1962, pp. 132–45; 1931). Wigforss argued that the remedial policies he advocated would create employment by freeing the economy from the fetters which private capital had placed upon it; they would free people from economic servitude and make them masters of the economy they had built. "That," Wigforss concluded, "is the true *economic freedom* that is a worthy goal for human beings to strive to attain" (1941, p. 320).

Insofar as Wigforss is known in the English-speaking world, his reputation rests largely on this prescription for overcoming the effects of the Great Depression. Wigforss consciously instituted an effective counter-cyclical program four years before the publication of Keynes' *General Theory*, thus making Sweden the first country to adopt modern stabilization policies. During his tenure as Finance Minister the Social Democratic government guided Sweden to an economic recovery that contrasted sharply both with Roosevelt's limited achievements in the United States and with the totalitarian planning in Nazi Germany and Stalinist Russia. Despite his lack of formal training, Wigforss understood the new economic literature well enough to contribute to it, to defend it effectively against criticisms from such established academic economists as Gustaf Cassel, and to explain it satisfactorily to the general public. Gunnar Myrdal later wrote that Wigforss,

> though older than the rest of [our] generation . . . should rightly be counted into our group [the Stockholm School of Economists]. He never gave himself time to venture into original

> economic analysis, but he read the literature more copiously than any of us, he was thoroughly familiar with and in complete sympathy with all the new ideas of the time, and took a lively part in the discussions between us which ran parallel to our writings (Myrdal, 1958, p. 257).

One need only compare the ineptitude of the British Labour party's economic policy since 1929 and the party's consequent political setbacks to appreciate Wigforss' political contribution (cf. Martin, 1975). His success in harnessing the techniques of the new economics to traditional Social Democratic objectives underlay the Social Democrats' continued electoral triumphs.

As the economy recovered and as increasing military expenditures began absorbing the resources that might have been used for new social policy initiatives, the Social Democrats faced once again the problem of how to achieve their objectives in an altered political environment. This time Wigforss and Per-Albin Hansson chose to cooperate with private industry. In November, 1938, Wigforss argued to the Gothenburg Bourse Society that neither the working-class movement nor private capitalists could hope to suppress the other party altogether, that they should recognize this fact, and should cooperate to achieve their common interest–increased efficiency in production. He proposed detailed discussion between government and business on methods for increasing capital formation, exploiting natural resources and new technology, promoting exports, and avoiding recessions. He hinted strongly that so long as private capitalists operated efficiently, they had little reason to fear intervention from a Social Democratic government. The outbreak of World War II meant that this overture to private industry produced few practical results, but it foreshadowed the "Harpsund democracy" of the Erlander period and the close contacts between industry and the Social Democratic government under Gunnar Sträng's long tenure as finance minister. Wigforss insisted that public-private cooperation proceed on the state's terms, but in later years, when Social Democracy's political position was weaker, this cooperation could work to the advantage of private industry (Svenning, 1972, pp. 26–27, 48).

World War II compelled a full and effective use of economic resources and sharply raised the level of public expenditures. It showed that much higher levels of taxation were feasible and that economic planning could be effective. As the war drew to a close the Social Democrats looked forward to their *Skördetiden* or "harvest period," when they would convert the wartime economic apparatus to the production of social welfare. Wigforss was largely responsible for the two documents that outlined policy for the postwar period, the new party program of 1944 and the *Working Class Movement's Postwar Program* of the same year. The fundamental idea behind these programs was to transform capitalist society through economic planning and social welfare policy rather than by nationalizing industry. Economic planning for full and efficient employment would extend social control over the organization of production. The increased production that followed would underwrite the social welfare measures of the Gothenburg program and a new emphasis on more equal wages (*solidarisk lönepolitik*), the foundations of a "just distribution and higher standard of living" (*Arbetarrörelsens Efterkrigsprogram*, 1944–45).

Although the Social Democrats' economic planning remained relatively modest in scope (compared, for example, to French indicative planning), and industrial democracy made little immediate progress, the party did carry through the social policy reforms that resulted in the welfare state–educational and tax reform, health insurance, housing subsidies, child allowances, and the like. In the 1950s their bourgeois opponents began intimating that with these reforms the Social Democrats had eliminated the grounds for their own existence: they had abolished the impoverished proletariat by making it secure, affluent, and unideological. Consequently, the party ought to step aside and allow efficient bourgeois executives and politicians to administer the new society. In such a climate, how were the Social Democrats to distinguish themselves from their opponents? If they advocated state intervention in the economy only when capitalism failed, what was the state's task when patched-up capitalism was "succeeding?" Was there still a clear boundary between socialist and liberal ideals or did the welfare state represent the culmination of Social Democratic hopes?

Wigforss now made his last major contribution to the ideology of Swedish Social Democracy. He insisted that the socialist program had not been accomplished. Relying on the same principles of freedom, equality, efficiency, cooperation, and security that underlay the Gothenburg program, Wigforss called attention to enduring capitalist defects in the welfare state. The control of investment decisions and factory policy remained largely in the hands of a select circle of private individuals. Inefficiency and waste continued at excessively high levels in the private sector of the economy. Inherited wealth obstructed the emergence of a classless society. The emphasis upon material consumption, competition, prestige, and success polluted the psychological atmosphere. Wigforss told a 1962 May Day audience that the wage-earner

> cannot readily agree to that view of the welfare state that is now frequently put forth, that it should secure a minimal livelihood for all, but allow whatever goes beyond this to be won through each individual's or each group's asserting itself in an unlimited competition for status, wealth, and power. That spirit clearly conflicts with the ideas that both the trade union movement and social democracy seek to follow in their public labors . . . that it still shall be the labor movement's ideas of equality, cooperation, and solidarity, that shall set their stamp upon society's continuing transformation (1962, pp. 149–50).

Toward the end of the 1960s these ideas formed the basis for the Social Democrats' new emphasis on equality, industrial democracy, and social control of investment.

By laying the ghost of determinist Marxism, by creating coherence between socialist principles and policy, by formulating central economic and social policies, and by appraising the deficiencies of the welfare state from a socialist perspective, Wigforss helped the Swedish Social Democrats adapt and renew their appeals over two-thirds of a century. His ability to integrate the claims of justice and economic efficiency with popular attitudes helps explain the Swedish Social Democrats' remarkable success in obtaining a consensual endorsement of their major economic and social welfare legislation (cf. Haskel, 1972). He is, by common consent, the most important contributor to Swedish Social Democratic ideology. Unfortunately, Wigforss himself never presented a single definitive and comprehensive statement of his ideas and thus it becomes necessary to draw together and examine both the socialist values that infused his thinking and the distinctive means he advocated to achieve his objectives.

The Aims of Social Democracy

By rejecting the notion that economic necessity leads ineluctably to socialism, Wigforss was logically committed to seek clarity about the values that a socialist society ought to realize. The more one trusted in irresistible economic tendencies, the more one inclined to the view that the working class "has no ideals to realize; it has only to set free the elements of the new society which have already developed in the womb of the collapsing bourgeois society" (Wigforss, 1970, p. 89). But if one believed, as Wigforss did, that the economic tendencies that ostensibly led to economic catastrophe and socialist revolution could and were being resisted successfuly, and that one need not wait for such apocalyptic events to begin the construction of socialism, then clarity about Social Democratic values and objectives was essential. Similarly, if popular enthusiasm for socialist ideals was a necessary condition for the construction of a socialist society, it behooved socialists to make these ideals vivid and appealing. What was it that made a socialist society morally superior to any bourgeois capitalist society? What values would have to guide its construction?

Equality, freedom, democracy, security, conscious control of the economy for increased and more efficient production, and solidarity form the essential basis of Wigforss' socialism. The objectivity of these values cannot be demonstrated in a scientific manner. Unlike R. H. Tawney, Wigforss did not rest his values on Christian foundations. He regarded values as choices which people must make according to their own conceptions of "right" and "wrong" and which they must defend with the best arguments that they can muster. He repudiated the *värdenihilism* (value nihilism) of the Swedish philosopher Axel Hägerström and the view of the Viennese logical positivists that moral discourse is "nonsensical" because moral propositions are neither true nor false (1941, p. 22; 1962, p. 122). Ethical postulates have a character that is necessarily provisional and tentative, Wigforss concedes, but this fact does not rule out the possibility of legitimate moral appeals. Conceptions of ethics and justice are linked with class, and thus until the advent of a classless society, a generally accepted moral system is unlikely, but in the meantime members of the working class and persons of good will can be expected to respond positively to Social Democratic ideals (1962, p. 123; 1941, p. 13).

Among Social Democratic values *equality* assumes the central place. To strive for equality is to strive to make it possible for everyone to live a valuable human life, to share in the freedom, authority, wealth, security, fraternity and opportunities for personal development that a rationally organized and solidaristic society makes possible (Wigforss, 1964, pp. 434–35; cf. Myrdal, 1971, p. 14). The social expression of such equality is the classless society:

> Socialism does not rely on an assumption of natural equality among human beings. Socialists have not failed to notice that not all human

> beings are equally attractive or strong, equally musical or intellectually gifted, equally energetic or moral. But equality can still exist. The differences that now create *social classes* can disappear. It is a combination of economic inequality and inequality in upbringing that holds these breaches open. Classes stem from social organization rather than natural differences (Wigforss, 1938, p. 4).

The classless society does not demand "absolute" equality of incomes and property. One can deviate from strict equality to reward achievement, to satisfy needs, and in some cases to stimulate production (Wigforss, 1952, pp. 27–28). The steelworker's overtime and the secretary's savings account do not violate the egalitarian principle. Wigforss does not insist on a single standard of distribution, such as equality or need or merit; the overriding objective is to prevent the formation of social classes and the resultant obstacles to personal development.

Classes exist when occupational differences reflect inequalities of wealth, income, and general education, *"inequalities that make it difficult for the different groups to come in contact with one another"* (Wigforss, 1956, p. 10, emphasis added). For Wigforss, as for R. H. Tawney, the major appeal of the classless society is that it removes conventional and emotional barriers between people. The model of equality is the unconstrained intercourse of comrades, be they school chums, workmates, or drinking companions. "The equality that I seek lives in every circle of comrades; for example students, even though one is not exactly like another." Fraternity, not identity, is the fundamental objective of equality (1941, p. 182; cf. 1964, p. 273; Tawney, 1964; Terrill, 1973).

The conception of equality as, in part, an equal chance to develop one's personality presumes a second of Wigforss' Social Democratic values: *freedom.* The demand for equality is a demand for equality in freedom as well. "Freedom" is a collective name for an innumerable variety of particular freedoms: to move about freely, to dress as one chooses, to speak one's opinion openly, to choose one's vocation, to participate in critical economic and political decisions, and so on. Both the liberal bourgeoisie and Social Democrats agree that "freedom" in general is a good thing; the debate between them begins with the question of *which* of these freedoms take priority—civil liberties? the consumer's free disposition of income? entrepreneurial independence?

As a liberal Social Democrat, Wigforss argues that the bourgeoisie are not the only defenders of civil rights; Social Democrats express equal concern for political liberties. He observes that liberal enforcement of civil rights has been less impressive than liberal rhetoric about the value of such rights. The two camps do not differ in principle, and in practice, Wigforss believes, Social Democrats should improve on the previous liberal performance. Like Eduard Bernstein, Wigforss (1941, p. 283) sees socialism as the logical extension of liberalism rather than its antithesis:

> Social democracy has never denied, but on the contrary considered it an honor to have its roots in the same intellectual soil that nourished the old liberal ideas of freedom. But it never saw the way to realize these ideas as being competition between individuals, regulated only by the free market and with the state's and the society's tasks reduced to a minimum.

He merely extends Kant's liberal doctrine when he states (1970, p. 95) that "all the individual's freedom that can be united with the maximum freedom of others, socialism will preserve."

Bourgeois opposition to the Social Democratic program has centered on the purportedly evil effects of state interference with the economic freedoms of the citizen. Conservative critics are quick to argue that taxation abridges the citizen's freedom to consume what he or she wishes; the higher the tax, the greater the infringement. Wigforss has two replies to this argument: first, all societies must tax to some degree; under Social Democracy tax levies result from democratic decisions and they purchase benefits in the form of social services. Here he side-steps the argument about freedom in favor of emphasis on the democratic and utilitarian character of the tax program. Wigforss' second reply rests on the economic principle of the diminishing marginal utility of income. If, he says, one can properly speak of an overall freedom of consumption, then a progressive tax structure increases the overall level of freedom of consumption. The fiscal infringement on a wealthy consumer's liberty is more than compensated for by the increase in the poorer consumer's freedom that comes from the assured provision for basic needs. "A more equal distribution of wealth increases the collective welfare, creates greater security for the mass of the population, and provides the material circumstances for a richer human life. The interests that are advanced by this policy are greater than those that are shunted aside, and that is enough" (1928b, p. 14).

An even more fundamental disagreement between Liberalism and Social Democracy concerns the freedom of the individual economic producer from public restraints. Wigforss does not contest the core of the liberal idea: that free choice of profession, the opportunity to determine one's economic fate, to exercise initiative and responsibility, to organize one's own work schedule, and to take risks and experiment, constitute important elements in the quest for a satisfying human existence. What he objects to is the fact that capitalist societies limit the practice of these freedoms to a tiny segment of the population. Property in the means of production affords the bourgeoisie these liberties, but modern industry reduces the factory workers to a propertyless and dependent condition. If laborers are to enjoy a modern version of the liberal values of productive freedom, economic independence, and active participation in their work, if they are to be liberated from dependence upon the vagaries of a market economy and freed from subjection to the arbitrary economic power of their employers, then the state must intervene on their behalf—"even if the individual may thereby be deprived somewhat of his precious rights, both to become a millionaire and to starve to death" (1970, p. 26). State intervention is required so that people need not bow to the unconscious working of economic forces. Subduing the forces of nature has given mankind a sense of greater freedom and, similarly, dominating and consciously controlling the economy rather than blindly submitting to its fluctuations will enlarge the sphere of human freedom.

Just as liberal democracies have traditionally established curbs upon concentrations of political power, so Social Democracy must now restrict excessive economic power to achieve freedom in the economic sphere:

> Overweening economic power (*ekonomisk övermakt*) in the hands of one limits freedom for the other. A more egalitarian distribution of power and influence within the economic realm increases freedom there, in the same way that universal and equal suffrage did in the political realm (Wigforss, 1967, p. 125).

Wigforss does not hide the fact the privileged classes will have to yield some of their former freedoms:

> The struggle for greater freedom for the worker in the work-place, in local politics, in national politics, has naturally been a struggle to limit the power and freedom of the previous ruling classes. The continuing work to eliminate economic privilege has the same goal. In this sense Social Democracy's strong position undoubtedly constitutes a threat to certain groups' freedom. That freedom must be limited still further so that freedom in like measure will be the inheritance of all people (1944, p. 34).

An individual's freedom always depends on others' freedom, and where an aristocratic distribution of freedom prevails, the power of the aristocrats will have to be limited so that freedom can be extended to others.

This discussion of equality and freedom naturally leads to a third of the Social Democratic values that Wigforss champions: *democracy*. If freedom is to be extended equally to all, and if freedom entails rights of participation and codetermination, then the fundamental elements of the democratic idea are already in place:

> The democratic idea that carried me over into the socialist party was the demand for equality for all of the members of society. Equality of civil rights, equality also in the question of possibilities to take part both in spiritual and material culture. That this equality simultaneously implied equal freedom for all to develop their talents and make their contributions was so self-evident that it hardly paid to mention it (1964, pp. 149, 272).

For Wigforss the essence of democracy is not parliamentary government or an opportunity to choose between competing elites, but freedom and equality.

Historically democracy had emerged from the struggle against monarchical absolutism. The two major lines of this struggle comprised distinct but complementary elements of the democratic idea. One feature of the effort to curb absolutism was the transference of power to representatives of the "popular will"; a second was the limitation of sovereign power through the extension of legal rights to the citizenry (Wigforss, 1941, p. 65). Social Democrats, however, cannot allow this struggle to be confined to the political sphere. Wigforss consistently stressed measures to limit the authority of industrialists and to increase popular participation, influence, and responsibility within firms. Economic power free from control or responsibility conflicts with the democratic idea; its exercise should be both subjected to public regulation and dispersed among the citizenry to a far greater extent than at present. Men and women ought to have the opportunity to cooperate in the management of enterprises. The conservative utopia of a "property-owning democracy" does not meet these requirements, for transforming citizens into small shareholders does not necessarily bestow upon them control and influence within large modern corporations. On the same grounds public ownership of industry is not a panacea; nationalization may, but need not, entail greater democracy within the firm.

An unregulated capitalist economy cannot guarantee a fourth important Social Democratic value: *security*. Indeed, the substance of this value can only be understood by contrasting it with the insecurity of the worker's livelihood in capitalist society. Deprived of ownership in the means of production and possessing little in the way of savings, subject to the perverse fluctuations of the labor market, and often exposed to the threat of debilitating injury at the workplace, the worker's existence in early capitalist society was precarious in the extreme. For the first generation of Swedish industrial workers, Wigforss writes, security was equivalent to the right to work, but his own conception of security extends well beyond the earlier view, to include public pensions, children's allowances, insurance against injury and sickness, and provision for adequate housing. A civilized society ought to provide an assured standard of living for all of its citizens, even those who might not be able to contribute actively to economic production.

Wigforss' discussion of security merges with his criticism of capitalism for its inefficiency at both the macroeconomic and the microeconomic levels. Capitalism not only produces an inequitable distribution of wealth, power, freedom, and security: but also it further compounds the problem of human welfare by producing below the level of production that is technically feasible. Contrary to liberal assertions, an economy based on private property, free enterprise, competition, and regulation through the price mechanism does not lead to maximal production. Rather, such an economy is characterized by periodic crises, underemployment of labor and resources, and waste resulting from class conflict, superfluous competition, and advertising. In the late 1920s, long before John Kenneth Galbraith took up the cudgels, Wigforss argued that the advent of advertising had led to the proliferation of useless, unnecessary, and trivial products while pandering to less attractive and desirable human impulses (1941, p. 137ff.).

Given the inefficiencies of a private capitalist economy, the "raising of the working classes's standard of living . . . surely cannot be effected without vigorous public intervention in our economic life nor without the active participation of the working class in the organization of production" (Wigforss, 1941, pp. 94, 140). Social Democrats cannot confine themselves to distributional reforms, but must steer the economy towards higher production and greater efficiency. Only through *conscious control of the economy for increased and more efficient production* can resources be exploited most effectively and material prosperity increased at the fastest possible pace.

These communal efforts for greater economic security and more efficient production derive from and reinforce the sixth and last major value that Wigforss stresses: the aspiration toward what may be called *cooperation, solidarity, community,* or *fraternity*. The new socialist society will strive to realize the quality implicit in the Latin root of the word *socialism: socius,* comrade. In place of the individual acquisitiveness of capitalism it will establish cooperation; in place of the excessively competitive atmosphere, sympathy, solidarity, and fraternity. It will endeavor to create conditions that allow an outlet for non-economic human impulses that are shackled by the present system of production, i.e., a sense of cooperative enterprise, comradeship, and social feeling unmarred by the competition for prestige (cf. Titmuss, 1971).

This sense of community influences the way the new society will determine policy. It will not merely count votes to come to a decision, but will weigh the intensity of opinions. In 1916 Wigforss wrote:

> It is only expressing an old lesson from every assembly that acts in a solidaristic way to say that one considers the intensity of the opposition, weighs it against the strength of one's own convictions, and does not with a weak and hesitant majority force upon a violently opposed minority a decision which requires everyone's strict cooperation for its execution (1941, p. 37).

The Social Democratic "politics of compromise" do not rest upon constitutional devices so much as upon sentiments such as these; indeed it is questionable whether they can function effectively in the absence of such attitudes. Wigforss' vision of an ideal democratic community embodies more than a set of formal relationships; it assumes a distinctive psychological atmosphere which is anticipated at present by the feeling of comradeship within a happy family or among a circle of close friends.

Even if one grants the desirability of each of these six values individually, questions may still arise about their mutual compatibility. Wigforss allows for possible conflicts at the margin among the socialist values. Just as all of an individual's desires may not be compatible, so a society may not be able to achieve all of its aims concurrently, even if one does not consider at all the difficulties of putting these aims into practice. Too great an emphasis on any one value, he warns, is to be avoided; instead one must construct provisional utopias that strive for the best possible balance. On the whole, however, the compatibility of the Social Democratic values is great and the conflicts among them minor; it is the task of Social Democracy, Wigforss wrote in a 1946 article on "Personal Freedom and Economic Organization," "to show that it is possible to combine the aspiration for justice and equality, planning and efficiency, with all the civil liberties that Western democracies rightly consider indispensable" (1967, p. 102).

Throughout his life Wigforss adhered to the principle that the task of Social Democracy was not merely to integrate the working population into Swedish society, but to transform bourgeois society by transferring economic power from capital to labor. He returned again and again to two fundamental criticisms of capitalist society based on his socialist values: First, its patterns of distribution were grossly unjust. Wealth, income, authority, freedom, status—in fact all the desirable fruits of social living—accrued disproportionately to the owners and managers of industrial production. Second, the capitalist economy operated inefficiently, creating unnecessary insecurity and competition. His demands for a more equal distribution of resources, for increased and more efficient production, and for industrial democracy—the constant core of his political program—flowed naturally and logically from these criticisms.

Wigforss' emphasis upon equality, solidarity, and conscious control of economic production, and his insistence upon their compatibility with freedom, democracy, and security, provides the theoretical basis for his (and his party's) vision of Social Democracy. Principles, not historic forces or pragmatic manipulation, guide Social Democracy; the basic idea of his 1944 party program was that "Social Democracy has the task of transforming society after these *principles*, which we can briefly term democratic" (*Protokoll*, 1944, p. 69). Abstract ideals, however, form only the beginning of a political program; one has to clarify the ways in which they can be embodied in institutions and policies. "General ideas such as freedom, justice, etc.," Wigforss writes, "have no comprehensible content other than in concrete contexts and . . . the forms they clothe themselves in change with alterations in what may be called, for the sake of brevity, the technical conditions of mankind's life in common" (1952, p. 141).

The Instruments of Social Democracy

The early Swedish socialists were persuaded that socializing the means of production was *the* socialist remedy for the ills of capitalism. Socialization served as a magical formula for eliminating all the malign features of capitalist society:

> It was thought that in a single blow socialization would strike down all the evils that the oppressed masses were rising up against and provide solutions to all the essential problems that the socialist criticism found in a capitalist economy (Wigforss, 1952, p. 11).

Little thought had been given to the specific forms public industry would assume, but few socialists doubted that socialization would effect at least eight important changes. It would: (1) eliminate class differences based on property ownership, simultaneously establishing a classless society; (2) abolish glaring extremes of poverty and wealth and institute greater economic equality; (3) terminate class conflict by removing the causes of strife and providing a basis for genuine solidarity; (4) end the exploitation of labor; (5) subject the economy to democratic control; (6) ensure economic security; i.e., a guarantee of employment or income; (7) produce rapid increases in the standard of living for the less privileged; and (8) solve the psychological problems associated with industrial labor (Wigforss, 1941, p. 106; 1952, p. 11).

The precise mechanisms by which the socializing of industry would accomplish these ends remained unspecified, but as long as the trend toward concentration of capital continued and the party remained sufficiently far from the seats of power, ritual incantations of the socialization panacea could take the place of a precise program. When it became clear, however, that small capitalist enterprises were not vanishing altogether and that the Social Democrats might come into office before the capitalist economy had ripened fully for the plucking, the strategy of socialization had to be considered more carefully. One of Wigforss' major achievements was to develop the doctrine that socialization was not a panacea, nor even an end in itself, but only one means that socialists might employ in constructing a decent society (1941, p. 104ff.; 1944b; 1948).

As a practical politician, Wigforss recognized the political hazards of the socialization policy. Proposals to transfer enterprises into public hands aroused enormous political opposition. Bourgeois denunciations of sociailist intensions to expropriate all species of private capital consistently evoked a vigorous response. Wigforss experienced first-hand the political difficulties of effecting even modest socialization when in the mid-1930s he proposed state gas and coffee monopolies to help pay for increased pensions. The political costs of such initiatives, together with his personal democratic conviction that the deeper the governmental intervention, the greater popular support ought to be, impelled Wigforss to seek alternative strategies.

He proceeded to demonstrate the weaknesses inherent in the party's excessive dependence upon the socialization strategy. The socialization of family farms and small businesses was not only impolitic, but unnecessary, because this modest private property placed the means of production in the hands of its operators. The nationalization of large enterprises could produce excessive state power, an outcome uncongenial to a man with Wigforss' liberal and syndicalist leanings. Above all, socialization could never be a sufficient strategy because the major flaws of capitalist society resulted not merely from private property rights (as the socialization strategy assumed), but also from excessive reliance upon market mechanisms. To socialize industry without organizing markets could result in an economy as erratic, inefficient, unjust, and undemocratic as capitalism.

There was, then, no single, simple solution that was both politically feasible and theoretically adequate for treating the ills of capitalist society. Wigforss concluded that in lieu of (or as a prelude to) socialization—an ambiguity that may have been intentional—Social Democrats should propose a series of measures that would quietly but steadily erode capitalist domination of the economy. Like Östen Undén and Nils Karleby, Wigforss regarded property not as an indivisible unit that must be socialized *en bloc,* but as a bundle of rights that could be transferred piecemeal to society and to employees. A gradual paring away of capitalist prerogatives, a creeping socialization, should be pursued; for it was not formal rights of ownership, but actual power to control, that was decisive. *Social welfare policy* and *progressive taxation* could begin to redress the distribution of national resources, undermine capitalist control of the flow of incomes, and socialize consumption of vital goods. *Economic planning* would organize the economy for higher and more efficient production, simultaneously breaking the capitalist monopoly over investment and production decisions. *Industrial democracy* would transform direction of the individual enterprise without changes in legal ownership. Together these measures would achieve the equality, freedom, democracy, solidarity, security, and economic efficiency that Social Democrats desired, even if formal ownership and some powers remained in private hands—and if these steps proved insufficient, *socialization* stood in reserve as a possible instrumentality.

Social welfare policy aims to redress the distribution produced by unregulated markets. Although Gustav Möller is generally considered the major architect of Swedish social welfare policy, Wigforss' Gothenburg Program, Postwar Program, educational proposals, tax reforms, and employment policy views give him an equal claim to this title. Wigforss explicitly rejected the notion that rapid economic growth could itself accomplish the Social Democratic objectives of abolishing poverty and creating a more just and egalitarian distribution of welfare. Poverty was not absolute, but relative to others' standards of living, and therefore "increased production means higher welfare for the whole population, only if it is distributed in a reasonable manner" (1928a, p. 3). Enlightened public policy ought both to insure people against the risks of life in industrial society and to remove certain critical commodities like education and health care from the domain of the market by providing them as public services. Like Gunnar and Alva Myrdal, Wigforss believed that such expenditures did not constitute a burden upon the economy, but were instead a sound investment in human resources.

Wigforss' initiatives rested on a clear distinction between liberal and Social Democratic social welfare policy. Liberals (whose cooperation in useful endeavors Wigforss endorsed) tried to alter distributional patterns to benefit the least advantaged, but avoided challenges to private property. Social Democrats, by contrast, sought to exert public control and extend public property rights in order to achieve their redistributional aims (Wigforss, 1938b, p. 13). They advocated progressive taxation to finance public programs:

> All the measures for education and health care, for sickness and accident insurance, unemployment insurance and old-age pensions can be considered as an effort to distribute prosperity more evenly, *at least to the degree that recipients of larger incomes must yield up through progressive taxation a portion of their incomes for the benefit of the less well-off* (Wigforss, 1941, p. 194, emphasis added).

Not only incomes but also property must be subject to progressive taxation. Wigforss envisioned sharply progressive taxes on capital and inheritances as at least a partial substitute for the traditional policy of equalizing property through socialization.

In discussions on the financing of social welfare policy, Wigforss emphasized that a generous policy, even when progressively financed, demanded a flourishing economy. "Becuase Social Democracy works for a more equal and more just distribution of property and incomes, we never forget that one must produce before one has something to distribute" (1944, p. 20; cf. 1935, p. 15). A high level of production, efficiently organized, in turn demanded economic planning, (*planhushållning*). Left to itself a private (or even a socialist) economy would produce well below its potential, generating unemployment, inefficiency and waste. On the other hand, planning without socialization might effectively organize and exploit society's productive resources.

"*Planhushållning,*" Wigforss wrote, "means a coordination of all the different forms of economic activity, public and private; . . . it is partly a different line from that which is expressed in the word socialization" (1967, p. 81). Private economic production perhaps did not need to be abolished, but it must be socially controlled, planned, and coordinated. In his influential address to the party congress in 1932, Wigforss argued that it meant little whether the state owned a particular factory—what was critical was the state's ability to organize the economy as a whole (*Protokoll,* 1932, pp. 472–73). Again at the 1944 congress he stressed the primary importance of economic planning rather than socialization:

> Whether an economic enterprise rests on private ownership or various forms of collective ownership, it must be coordinated in an overall economic plan, if labor and material resources are not to languish in idleness or inefficient production (*Protokoll,* 1944, p. 41).

Control, not ownership, of industry was decisive.

Private firms can and will plan the economy, Wigforss continued in a striking antitipation of Galbraith's thesis in *The New Industrial State,* and therefore Social Democracy must demonstrate its capacity to do so. The economic planning Wigforss espoused was not detailed, centralized planning of the Stalnist variety. It did not require the abolition of the market; markets could perform useful functions. They were, for example, more effective than price controls in moderating inflation, Wigforss remarked toward the end of World War II (*Protokoll,* 1944, pp. 272–73). (Lest anyone suggest that the persistence of markets was incompatible with socialism, Wigforss pointed out [1941, p. 184] that capitalist society contained "pre-capitalist" peasant and petty bourgeois remnants; why then might a socialist society not similarly contain "capitalist remnants?") Partly as a result of Wigforss' urging, the scope of Swedish planning expanded steadily from the 1930s when it centered on the management of aggregate demand to obtain full employment, to the war years with more detailed macro-economic planning, to the post-war emphasis on labor market planning and some abortive efforts at microeconomic planning, to the concentration in the 1960s on greater social influence over investment priorities. Wigforss supported these initiatives as steps toward social control of economic organization.

A thorough investigation of the development of Swedish economic planning is inappropriate here, but Wigforss' pioneering efforts in the 1930s to plan for full employment deserve brief mention. Wigforss traced the depression to shortfalls of consumption and investment. In addition to the reluctance of private consumers to spend, the managers of industry lacked the enterprise and initiative necessary to undertake the requisite amount of investment. As a result savings exceeded investment, capital formation fell below desirable levels, and employment declined. In such circumstances true "economy" demanded that resources no longer go unused. The proper policy was obvious already in 1928:

> If there is not sufficient "enterprise" to employ savers' unused purchasing power to put men to work, then thrift in the public sector would appear at the moment to be a dubious virtue. For this reason it is an old adage that during depression society ought to seek a remedy for deficient private capacity or willingness to buy. State and local government in times of depression ought to commence desirable public works without delay. When private enterprises hesitate to put savings to work, then the public sector must step forward and direct their use (1941, p. 293).

This remarkable passage with its emphasis on the need for public initiatives to maintain a satisfactorily high level of investment, foreshadows not only the anti-depression policies of the 1930s, but also more vigorous public initiatives to improve the workings of the economy as a whole.

Once full employment was attained, it was possible to attack underemployment by expanding the labor force and placing it more efficiently through active labor market policy (Öhman, 1974). Once the general level of investment had been regulated, Wigforss could turn his mind to the task of achieving greater social control of business investment. He recommended the accumulation of public capital funds under state control; the Social Democrats' willingness to use the national pensions funds for industrial investment and the "Meidner funds" proposal represent belated recognition of this suggestion. Thus both in the labor market and in the capital market the conception of economic planning broadened and deepened under Wigforss' stimulus.

The measures surveyed hitherto do little to alter conditions for the worker on the job or to democratize the firm. Here the essential instrument is industrial democracy. Until working men and women participate actively in the direction of their daily work and the formation of other pertinent policies of the firm, Wigforss stressed, social democracy would not be a reality. Wigforss was never dogmatic about the precise forms that industrial democracy would assume—or, to speak less charitably, he was never quite certain what forms it should assume. The necessary structures must arise from the workers' need for greater freedom and independence in their work, Wigforss argued. The natural channel for this demand is the union, but the arrangements that unions would work out with management would vary from case to case. Employers might also institute programs with real merit; their interests in increased production could at times coincide with workers' interest in greater autonomy on the job.

The essential criteria for genuine industrial democracy were two: First, the workers must have power to make decisions, not merely to consult. Wigforss wanted workers to have *bestämmande rätt*, not *medbestämmande rätt*—the right to determine, not to codetermine with management (*Aftonbladet*, 3 January 1977, p. 23). Second, decisions for improved production must consider the worker as an autonomous human being, not as a commodity to be disposed of at the will of the employer. Well-ordered systems would also allow for representation of consumers' interests, though just how Wigforss never explained. Through experimentation, structures could be devised that would generate both more efficient production and greater psychological satisfaction for workers (Wigforss, 1922, 1970).

The measures already outlined might seem to make *socialization* of the means of production superfluous, but Wigforss showed a marked reluctance to draw this conclusion. Demands for socialization of natural resources, energy sources, transportation, large banks, and insurance companies appeared in the Gothenburg Program. As he neared office, however, Wigforss lessened the emphasis on these points. In a 1927 article, "Hairsplitting about Socialism," he ventured the view that socialization was not an absolute concept, that it was a question of degree, not of polar opposites, that there were various criteria for socialization other than common ownership; e.g., production for need, a more egalitarian distribution of results, and common administration of the economic enterprise. In a conciliatory speech to industrialists in 1938 he went still farther, conceding that private management sometimes might be more efficient than public, that no single form of economic organization could be regarded as the only suitable one under all conditions, and that private investment would be a perfectly satisfactory, even necessary, alternative to some public investment. As the economy recovered from the depression, demand could reassume its old forms and "a more far-reaching regulation of production from the public side would not need to be considered" (1941, pp. 131–34, 332–43).

Toward the end of World War II and after leaving public office Wigforss increasingly sounded a more radical note, pointing out that private managers could conceivably frustrate the operation of Keynesian policies by refusing to invest or by transferring capital abroad. He stressed the utility of an expanded public sector for obtaining full employment with price stability, control of investment, greater equality, and industrial democracy (1952, pp. 59–60). This ambiguity and apparent inconsistency on the socialization issue may have stemmed from his conviction that bold measures required more public support than was available while he was in office.

Wigforss did not object to private property per se, but to concentrations of private property that gave certain classes and individuals the right to restrict others' freedom, independence, income, or intellectual development. If it became necessary or attractive to employ socialization to curb these excesses of private economic power, then more sophisticated forms of public enterprise than state capitalism merited examination. In particular Wigforss advocated experimentation with *"sämhällsföretag utan ägare,"* "public enterprises without owners." Such enterprises would operate as public foundations. Their sole obligation to providers of capital would be the payment of a fixed rate of interest. Workers would cooperate actively with management in the administration of the firm. Power would no longer belong to a few private individuals, but would rest with employees, a socially responsible managerial group, and possibly consumers. In this way the still unsolved problem of accumulations of economic power by a small minority—of private or public figures—might be solved, Wigforss argued (1962), and the institutional foundations of a genuinely democratic socialism be established.

Unlike some recent Social Democratic theorists, then, Wigforss did not totally eschew socialization as a possible means of implementing socialist values. What he did reject was the notion that socialization alone could realize socialist aims.

Wigforss' Social Democratic strategy, then, is a policy of permanent reform. He rejects the charge that successful reformism diminishes the prospects for more radical transformation. Through initiatives for social welfare policy, progressive taxation, economic planning, industrial democracy, and socialization, the party can simultaneously raise the employee's standard of living and restrict the capitalist's privileges. Each series of reforms sets the stage for new ones and weakens the capitalist's ability to resist. Unlike Gunnar Adler-Karlsson (1970), Wigforss does not toy with the notion of simply creating a balance of social forces. Wigforss is unambiguous; through a process of gradual reforms, he wants to transform bourgeois society and to establish social control of the means of production.

Conclusion

For much of this century Ernst Wigforss defined the ideological cleavages of Swedish politics. He pleaded for Social Democracy as an alternative to liberal welfare statism and to state socialism. He noted that the liberal welfare state extended a safety net to protect its citizens from dire poverty, and strove to maintain full employment, but otherwise left the inequalities of bourgeois capitalist society intact. Large fortunes still served as the basis for class distinctions and the administration of enterprise remained oligarchic. This liberal welfare state, Wigforss argued, could and should give way to a classless society, a Social Democracy characterized by general equality of condition, social planning of investment and economic development, and industrial democracy. This vision of Social Democracy regards socialization only as a possible means, not as an absolute necessity for socialism, and shows no inclination to abolish wage labor, but its emphasis upon equality, democracy, and public control of society's economic resources reflects socialist values.

Wigforss' Social Democratic vision provoked both bourgeois and left-wing opposition. While in office, Wigforss bore the brunt of bourgeois attacks on Social Democracy. His proposals for progressive taxes provoked cartoons picturing him as a thief with his hands in the pockets of solid citizens. His initiatives for economic planning, bourgeois critics claimed, led down "the road to serfdom." These opponents misperceived the character of Wigforss' socialism; they believed that any radical socialist must advocate dogmatic state socialism and centralized planning, and therefore they attributed this position to Wigforss. His death in January 1977 provided an occasion to concede that much criticism of him had been distorted and that his prescriptions, while exceeding their notions of the proper degree of equality and public intervention, had not been malevolent in intent.

Swedish Communists and some left-wing Socialists regarded Wigforss' failure to advocate immediate and far-reaching socialization as evidence of insufficient radicalism. They refused to accept Wigforss' argument that traditional socialization on the state capitalist model only transfers property rights from private to collective owners and neither ensures that the exercise of these rights will promote freedom, equality, democracy, security, and solidarity, nor regulates the erratic workings of the market economy that frustrate macroeconomic efficiency. They failed to realize that the worker was subordinated not only to the personal authority of the private manager, but also to the vagaries of a market economy characterized by large accumulations of capital and the imperatives of large-scale production. They did not distinguish between subjection to capital in the sense of private property and subjection to capital in the more technical economic sense of accumulated productive assets. Socializing industry could eliminate dependence upon private managers, but not subjection to market forces and bureaucratic organization. To remove or reduce the latter sort of dependence requires economic planning, social welfare policy, and industrial democracy. One of Wigforss' great achievements was to discern this fact and to devise appropriate remedies.

The most effective criticism of Wigforss arises within Social Democratic ranks. It is that Wigforss failed to create a provisional utopia sufficiently concrete to define the respective spheres of industrial democracy and democratic planning—to reconcile the direct democracy of the workplace with the representative democracy of the parliamentary system. Precisely this issue badgers the Swedish Social Democrats as they consider the Meidner wage-earner funds proposal: how much control over industry should rest with the workers? with consumers? with parliament? In a 1974 essay Wigforss acknowledged the problem:

> On the one hand one wants to steer total investment and national economic resources through society-wide economic planning; on the other the rights of control in individual

> enterprises are to be placed in the hands of the workers. . . . *How power should then be divided among the workers in the individual enterprises, and society in the form of a more or less centrally organized economic planning, and consumers over and above their power to buy or not to buy–that belongs to the many questions that one would like to see more closely examined* before proceeding with the opinion building that must become the next task for a Social Democratic politics, *when the movement can unite on a conception of economic democracy that will place the right to decide over production and its distribution in the hands of the people as a whole* (1974, pp. 393–95, emphasis added).

Wigforss' observation that the formation of opinion on this question should "rest on the self-evident premise that there is no other ground on which to build a socialist society than on the working person" (1974, p. 395) is not only not self-evident–it fails to recognize the interests of the retired and disabled, for example–but also completely useless for resolving such touchy issues as the proper institutional arrangements for representing the interests of consumers and citizens in general vis-à-vis workers in a particular firm or industry. Wigforss' rhetorical flourishes fail to disguise the ambiguity in the notion that control over the economy should be lodged *"i hela folkets händer,"* in the hands of the people as a whole. The lack of concreteness in Wigforss' vision is not irremediable, but the potential conflict between local direct democracy and national representative democracy is an unresolved problem in his program.

Despite this shortcoming, the influence of Ernst Wigforss' program for a Swedish road to socialism makes it necessary to abandon the view of Swedish Social Democracy as strictly "pragmatic" and unideological. Herbert Tingsten's thesis that the Social Democrats abandoned their socialist principles in favor of a welfare-state "over-ideology" must also be revised. Wigforss' continuing radicalism and the support it has found in the party and the trade union movement have forestalled an end to ideology in Swedish politics (cf. Lewin, 1967).

Because Wigforss' pronouncements have been so important in defining the ideological stance of Swedish Social Democracy, they also reveal much of what is distinctive in the movement–its emphasis on productive efficiency as well as distributional reforms; on transferring effective control (if not formal ownership) of the means of production to the public and its elected representatives; its optimism regarding scientific progress, rationality, and high levels of material production. His exposition of a Social Democratic politics based on the ideals of freedom, equality, democracy, security, solidarity, and economic efficiency could benefit both American politics and social science. American political life suffers from the lack of a coherent and practical reform program, yet the promising territory between the exhausted liberalism of the Democrats and the hazy projections of revolutionary Marxists, old and new, remains unexplored. American social science suffers from its failure to distinguish and assess the different objectives behind public intervention into private property and the market economy, and from its reluctance to evaluate the varying instruments employed to attain these objectives. Social Democratic theory may not provide a panacea for these problems, but careful consideration of it might both inspire new initiatives for a more decent society and improve understanding of current social policy. The ideas of Ernst Wigforss, trenchant in their critique of bourgeois economics, welfare-state liberalism, and doctrinaire socialism, offer a suitable starting point for this investigation.

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Social Theory and Revolutionary Activity in Marx*

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Economic determinist attempts to deduce specific political conclusions or strategies from Marx's general theory do not jibe with Marx's own extensive political activity. Instead, Marx's development as a political theorist and organizer passed from observation of (French Revolution, Chartism) or participation in (1848, the International Workingmen's Association) existing radical movements to formulation of new theories and strategies, and then to application of these strategies in subsequent movements. In applying his general theory to formulating strategies and historical explanations, Marx utilized a framework of mainly political auxiliary statements to define the specific international and national historical setting. Faced with the defeat of a strategy or a tension between the strategy and an older theory, Marx reformulated his theory in different ways, sometimes altering these auxiliary statements rather than the general theory to explain unexpected events, more rarely changing the general theory itself. These new explanations reinforced his strategies. This impact of political experience on Marx's thinking illustrates his famous definition, drawn from Theses on Feuerbach, *of revolution as a "practical-critical" activity. Taken as a whole, Marx's new explanations underline the role of politics in historical materialism and conflict with Marx's general expectation that economic oppression alone would ultimately drive the proletariat to make socialist revolution.*

1. Economic Determinism and Marx's Political Activity

As a revolutionary activist, Marx studied particular historical situations and novel political developments in order to further proletarian revolution. Far from reducing the unique aspects of his historical setting to universal economic forces which would "ultimately" determine events, he sought to learn how far and with what peculiarities the capitalist mode of production had taken hold, and to define specific potentials for revolution within each actual society. Far from reducing politics to a simple "reflection" of economic structure, he carefully observed the new political movements of his time, and devised political strategies that could realize the general potential for working-class liberation in a definite historical setting. Marx's own political activity exemplified his third and eleventh theses on Feuerbach: no "educator" or philosopher stands above history; rather, learning from historical experience, men—political men—must change their own circumstances. Before the revolution of 1848, Marx often emphasized political action and revolutionary defiance. Opposing Hermann Kriege's "true socialist" groveling before John Jacob Astor in New York in 1846, Marx commented: "Such a doctrine [Kriege's], preaching the voluptuous pleasure of cringing and self-contempt, is entirely suited to valiant —*monks,* but never to men of action, least of all in a time of struggle." Long afterwards, in 1871, he contrasted German "slavery to heaven" with the Paris Commune's "heaven storming": "History has no . . . example of similar greatness" (Marx and Engels, 1976, Vol. 6, pp. 49, 231; Draper, 1971, pp. 221–22; Schraepler, 1972, p. 160).

For most interpreters of Marx, especially those influenced by economic determinism, the shifting character of Marx's political judgments as responses to new situations and movements seems atheoretical (Hammen, 1972, pp. 680–81; Moore, 1975; Lichtheim, 1965, pp. 99, 89). In its narrower form, this view argues that for Marx the development of the productive forces straightforwardly determines all other social activities. Political analysis and action play no important role. In a more sophisticated form, an economic determinist may hold that once the appropriate economic circumstances arise, political activity plays an essential part. Thus, an economic determinist might argue, the ideas of a Diderot or a Rousseau and Jacobin political activity contributed to the achievement of bourgeois domination in France (Plekhanov, 1961, pp. 80–92). Even on this sophisticated view, however, the development of the economic forces constrains the effectiveness of political activity within narrow bounds and drives political movements to an "inevitable" result. The determinist theorist simply reads the next political step out of the existing economic circumstances and the concomitant political developments: the disarray of the old ruling class, the challenge to traditional ideas, and the appearance of a large democratic (or, under advanced capitalism,

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socialist) movement. For writers such as Lichtheim and Moore, the conflict between alternative political strategies presents no major difficulties. If one knows the economic forces, one knows politics.

Yet Marx's politics and determinist preconceptions consistently fail to jibe. In the 1840s Marx helped found the Communist League and led the radical wing of the German democratic revolution. A determinist might infer that at this time socialism could attain power only in advanced capitalist England, then nearing a proletarian majority; in Germany, only a vigorous democratic revolution, catapulting the bourgeoisie into power, could succeed (Blumenberg, 1972, p. 87; Fernbach, 1974, pp. 55, 58). Yet in the *Manifesto,* Marx looked to backward Germany as the setting for both a democratic and an "immediately following" proletarian revolution. According to economic determinism, Marx dismissed the peasants as a doomed and regressive force (Mitrany, 1951, Chs. 1–2). But Marx organized his first communist rally in Germany in 1848 among peasants in rural Worringen; his strategy pivoted on a unity between rural and urban democrats and communists. On a determinist account, the internal dynamics of capitalism should have promoted a strong English socialist movement by the 1860s, and the unexpected decline of Chartism had to derive from temporary economic improvement (Fernbach, 1972, p. 29). Marx, however, following Gladstone, argued that English capitalists had purchased their dazzling prosperity entirely at the expense of workers; the English proletariat had made no substantial gains since the 1840s. To explain socialism's poor following in England, Marx invoked the social and political division, nurtured by racism, between English and Irish laborers, which he called the "secret of the impotence" of the English working class, despite its relatively high level of unionism. Therefore the English ruling class knowingly spawned degrading images of the Irish "through press, pulpit and comic papers" (Marx and Engels, 1965, pp. 236–37). At Marx's initiative, the International Workingmen's Association (IWA) campaigned for the freedom of Ireland and the overcoming of this hostility within the working class. Determinist scholarship has offered no insight into this repeated disjunction between Marx's politics and its interpretation of his theory. Glimpsing Marx's political activity, these scholars either criticize Marx for inconsistency with this "rigorous" canon or ignore Marx's politics entirely (Moore, 1975; Shaw, 1978; Gilbert, 1976).

From his earliest writings, however, Marx condemned any dead scholasticism which isolated theory from practice and did not aim to "change the world." Taking Marx at his word, we should try to discern the theory in the political activity. Furthermore, Marx's politics might cast new light on the character of historical materialism. Perhaps these scholars have begrudged Marx his own more complex mode of historical analysis or of formulating political strategy.

In the second section of this article, I will distinguish between two types of theorizing in Marx and contrast his general hypothesis on the relations of modes of production and class struggle and his analysis of the dynamics of an ideal capitalism with his explanations or strategies in particular historical circumstances. The third section will show how Marx studied the French Revolution and worked out this second kind of theory, focused on the energetic role of a worker-peasant alliance, in preparation for the German democratic revolution of 1848; the fourth, how Marx refined this strategy in response to the revolution's defeat. In the fifth, I will illustrate the impact of Marx's second strategy, based on the experience of English Chartism, on a revolution in his fundamental theory in *Capital;* the sixth will discuss the political influence of *Capital* in the IWA. The conclusion will indicate the problems posed for subsequent radical movements by the tensions between Marx's two types of theorizing as well as those within his fully developed political strategies.

2. Marx's Two Types of Theorizing and the Concept of Auxiliary Statements

In his general materialist theory, Marx pursued two "guiding hypotheses." First, in the *Manifesto,* he argued that "all history is the history of class struggles." Second, to explain these political clashes, Marx looked to an underlying material foundation in the modes of production. In each entire social epoch, he argued, a determining relationship existed between the way men and women produced material wealth and their forms of political and intellectual life. Furthermore, contradictions between new forces of production (the mental and physical activities of the producers with existing raw materials and implements) and older social relations of production (the relations between exploiters or nonproducers and producers) generated new relations of production and forms of class conflict, and ultimately, new forms of society. Analytically, this general materialist hypothesis provided criteria for discerning the fundamental features of the newly arisen capitalist society: its enormous expansion of production, its vast uprooting of small property holders and dissolution of traditional customs, its revolution against feudalism. Beyond this, modern factory production forced a propertyless working class to associate. Marx's study of this material underpinning indicated the type of class struggle and new society which would probably spring from capitalism. Politically, Marx's theory provided an insight into the historical process through which small groups of radicals or isolated working-class movements could hope, despite immediate appearances, to become decisive revolutionary forces.

In articulating this broad pattern of history, Marx occasionally offered very general, abstract formulations as in his 1859 *Preface:*

> No social order ever perishes before all the productive forces for which there is room in it have developed; and new, higher relations of production never appear before the material conditions of their existence have matured in the womb of the old society itself. Therefore mankind always sets itself only such tasks as it can solve; since, looking at the matter more closely, it will always be found that the task itself arises only when the material conditions for its solution already exist or are at least in the process of formation (Marx and Engels, 1974, p. 183).

To interpret this statement, however, one would want to know exactly how Marx applied it to specific historical situations. If a social order develops "all the productive forces for which there is room in it" before it generates a new one, did the first major crisis of capitalism in 1857 already indicate that this system had grown old? Did Marx offer any guideline about how large a bourgeoisie must exist before a democratic revolution can take place, or whether a majority proletariat must exist prior to a socialist one? If not, how are we to interpret statements of this kind? Astutely comparing Marx's hypothesis to some theories in the natural sciences, Richard Miller (1975, p. 392) has argued:

> One needs to understand to what extent and in what ways his [a scientist's] statements are meant to hold in order to decide whether the description is true as intended or is an invalid oversimplification.... A geologist might say, "The history of the earth's surface is the history of continental drift." If he meant to explain "the general idea guiding his studies in plate-tectonics," we would accept his statement as a legitimate simplification of reality. This is true even though the earth was once entirely covered with water (hence, there were no continents), and, before that, was molten (hence, there was nothing like a continent).

Marx elaborated his guiding hypotheses in two separate and only partially related directions. In the first, he produced a new, remarkably sophisticated analysis of the general tendencies of an ideal capitalism in *Capital.* In the second, he worked out explanations and strategies for events and movements within definite historical settings.

The first reinforced the insights into the oppression of the proletariat and its political struggle to emancipate itself noted above. In forging this theory, Marx studied capitalist tendencies "abstracted" from any specific capitalism, even that of England (though as we shall see, *Capital* also influenced specific radical debates). Thus, in volume 3, he explained recurring capitalist crises in terms of a sharply qualified "law" or "tendency" of the rate of profit to fall. This tendency depended on a sufficiently rapid rate of introduction of machinery to offset increases in productivity (the rate of exploitation). Counteracting tendencies such as a cheapening of constant capital (machinery), foreign trade to countries with a lower level of productivity, or relative overpopulation could retard or offset this tendency (Marx, 1961, Vol. 3, p. 229). As Marx stressed, many real circumstances, including the economic and political struggle of workers against the capitalists, intervened between the analysis of the abstract "law" and an actual falling rate of profit (Meek, 1967).

Marx's second type of theory focused on specific situations: what strategy should German communists adopt in the democratic revolution in 1848? How might one explain the victory of the Second Napoleon in France in 1851? In analyzing these situations, Marx never concluded that general economic causes "necessarily" determined one political outcome rather than another. Instead he appealed to a specific combination of economic and political factors to define the alternatives in each upcoming case or to explain a unique result. Over time, alternate political strategies of the contending forces might potentially lead to dramatically different outcomes. While this second type of theorizing made use of arguments drawn from the first (criteria for beginning to analyze classes, for example), it examined a different, more complex object: the actual political setting of a revolutionary movement as opposed to the internal dynamics of capitalism (or earlier social systems). In applying the general theory to formulating explanations or strategies, Marx always modified it in certain characteristic, mainly political ways.

Now Marx distinguished between these two types of theoretical activity. In *Capital,* Marx underlined the vast array of "empirical" conditons which the general theory left out:

> The direct relationship of the owners of the conditions of production to the direct producers—a relation always naturally corresponding to a definite stage in the development of the methods of labor and thereby its social productivity—reveals the innermost secret, the hidden basis of the entire social structure, and with it . . . the corresponding form of the state. This does not prevent the same economic basis—the same from the standpoint of its main conditions—due to innumerable different empirical circumstances, natural environment . . . external historical influences, etc. from showing infinite variations and gradations in appearance, which can be ascertained only by analysis of the empirically given circumstances (Marx, 1961, Vol. 3, p. 772).

As Marx moved from working out his general theory to offering historical explanations or strategies, he took account of these "innumerable empirical circumstances," external influences, and the like. In his 1877 letters concerning a potential directly communist development of the Russian *mir* (agricultural commune), Marx compared dispossessed Roman peasants who became a "mob of do-nothings" to post-enclosure English farmers who became wage earners, and noted:

> Events strikingly analogous but taking place in different historical surroundings led to totally different results. By studying these forms of evolution separately and then comparing them, one can easily find the clue to this phenomenon, but one will never arrive there by using as one's master key a general historico-philosophical theory, the supreme virtue of which consists in being superhistorical (Marx and Engels, 1965, p. 313).

Historical reality disciplined Marx's materialist political analysis. No abstract or "superhistorical" account of social forces or general trends could legitimately override real circumstances.

During this period, Marx thought that a proletarian revolution in advanced Western Europe might precede a direct communist revolution in Russia. Thus, at a minimum, if one country passed through certain definite stages of production, it would condition the future development of others; followers on the international scene would not have to repeat the first country's history step by step. The productive dynamic of Marx's general theory in the *German Ideology* or the 1859 *Preface* indicated a broad trend without specifying the course which any given country must pursue. Therefore, Marx could expect socialism in Germany in 1848 and still defend his general pattern of history (Shaw, 1978, pp. 79–81).

In an 1885 letter to Zasulich, Engels also insisted that Marx's strategies required attention to particular circumstances: "To me the historical theory of Marx is the fundamental condition of all *reasoned* and *consistent* revolutionary tactics; to discover these tactics one has only to apply the theory to the economic and political conditions of the country in question" (Marx and Engels, 1965, p. 385). Yet this "only" is not so simple. Though Marx and Engels always followed the procedure which Engels described, they never closely analyzed the difference between these two types of theorizing.

The concept of auxiliary statements from the philosophy of science may clarify the relationship between Marx's general theory and his specific strategies and explanations. According to Hilary Putnam, a scientific theory gains acceptance through certain striking successes, as when Newton derived Kepler's laws from his theory of universal gravitation. The successful theory, however, confronts many problems which it has not yet solved (and some which may simply be too difficult to solve). In tackling these problems (as well as in the original successes) scientists never apply the general theory by itself, but only in conjunction with auxiliary statements which specify the context. Scientists regard these statements as much less certain than the theory under test; compared to the "laws" of the general theory, such statements are contingent or "accidental." To apply Newton's theory for example, astronomers may make a number of simplifying assumptions (auxiliary statements) such as: no bodies exist except the earth and the sun; these bodies exist in a hard vacuum; they are subject to no forces except mutually induced gravitational forces. In making predictions or offering explanations, scientists often do not spell out such auxiliary statements. When they use the word "theory," they hardly consider such statements to be part of it, a rough analogy to Marx's formulation of his guiding hypothesis or of the general "laws" of capitalism.

According to Putnam (1975, Vol. 1, pp. 254–59), general theories do not imply obvious predictions, and scientists rarely try to falsify an accepted theory which has had some significant practical successes. They discard an older theory as untrue only when glaring anomalies emerge, and someone offers a better alternative theory to explain them. Until then, scientists usually seek new auxiliary statements within the given paradigm.

Analogously, in applying his general theory, Marx used auxiliary statements to get at the context. Thus, to explain the weakness of English socialism in the 1860s, Marx stressed two kinds of auxiliary statement: the specific international political setting characterized by England's colonial domination of Ireland, and the role of political traditions, in this case, racist attitudes among English workers. Engels' 1890 letter to Bloch attempted to mark off specific historical analysis from "ultimate" economic causation in precisely this way. In explaining the rise of Prussia, Engels emphasized the importance of religious tradition, the Reformation, as well as of Prussia's international "entanglement" with Poland (Marx and Engels, 1975, p. 692).

Putnam notes a dialectic of prediction and explanation in the natural sciences which also exists in Marx. If a prediction fails within an established theory, scientists usually try to explain the unexpected result by adopting new auxiliary statements rather than by revising the general theory. When astronomers observed the deviant orbit of Uranus, for instance, they did not doubt the theory of universal gravitation. Instead, Adams in England and Leverrier in France simultaneously predicted the existence of another planet whose gravitational pull would account for the deviation. Astronomers subsequently discovered Neptune. The existence of this planet then served as a new auxiliary statement to correct the general theory's explanation of the orbit of Uranus.

In the 1840s and 1850s, Marx predicted that English workers would soon form a strong socialist movement. Despite the continuing oppressiveness of capitalism, they did not. Confronted with this failure, Marx examined the social and political divisions within the English working class more closely, and altered an auxiliary statement on the political consequences of English colonialism. This changed explanation led in turn to Marx's refined strategy on Ireland in the IWA, and held implications for the general internationalism of Marx's political strategy (Gilbert, 1978). This dialectic of explanation and strategy, given fresh political experience, also illustrates Marx's early thesis on Feuerbach that revolution involves practical-critical activity.

3. Marx's Strategy in 1848:
The French Revolution Radicalized

In its first sections, the *Communist Manifesto* drew a very striking picture of the rise of the bourgeoisie, its unheard-of economic transformation of the world, its political triumph, its forging of a propertyless proletariat, and finally the emergence of a working-class revolutionary movement against it. A reader might easily interpret Marx's argument as an example of a sophisticated economic determinism, and project a socialist revolution in 1848 only in advanced England. On the continent one might infer that Germany would generate only a bourgeois democratic revolution; European countries would not experience powerful nationalist movements; and the peasantry would play no significant progressive role. Yet in section 4 of the *Manifesto,* Marx envisioned a different strategy; proletarian revolution would come first not in England but in Germany whose population was four-fifths peasant:

> The Communists turn their attention chiefly to Germany, because that country is on the eve of a bourgeois revolution that is bound to be carried out under more advanced conditions of European civilization, and with a much more developed proletariat, than that of England was in the seventeenth, and of France in the eighteenth century, and because the bourgeois revolution in Germany will be but the prelude to an immediately following proletarian revolution (Marx and Engels, 1962, Vol. 1, p. 65).

Marx also stressed the importance of a Polish nationalist movement against tsarism, and regarded "agrarian revolution" as the "prime condition for national emancipation." In the first sections Marx saw the peasants as reactionary in trying to "roll back the wheel of history," and maintain their small properties, but revolutionary insofar as they envisioned their "future interests" as proletarians (Marx and Engels, 1962, Vol. 1, pp. 64, 41). In Poland and Germany, however, not even a democratic revolution had taken place. The remnants of serfdom still weighed upon the peasants who had yet to gain unencumbered title to land. How did Marx's earlier argument apply here? From an economic determinist standpoint, Marx's strategy in 1848 appears paradoxical.

In arriving at this strategy, Marx had studied the French Revolution, that "lighthouse of all revolutionary epochs," and adopted a number of auxiliary statements concerning the international situation, the internal dynamics of republican revolution and the role of a worker-peasant alliance. We should now trace Marx's gradual formation of this strategy in the early 1840s.

In his 1843 *Introduction to a Critique of Hegel's Philosophy of Right,* Marx assessed Germany's role within the international setting of Europe. Germany suffered from an uneven development of economics, politics, and philosophy. In the economic sphere, the German bourgeoisie lagged behind nineteenth-century Britain, and in political development behind

France in 1789. Germany had joined not in making but only in defeating modern republican revolutions. In this uneven development, an advanced German philosophy coincided with this comparative backwardness of capitalism and republicanism.

> The German governments combined bourgeois with feudal oppression, "the civilized deficiencies of the modern political order (whose advantages we do not enjoy) with the barbarous deficiencies of the *Ancien Régime* (which we enjoy in full)."

Marx suggested sardonically that Germany might soon reach the level of European decadence without ever achieving "European emancipation," like "a fetishist wasting away from the diseases of Christianity." Such combined oppression would drive the workers more swiftly to revolt; their rebellion would frighten the bourgeoisie into unity with absolutism (Easton and Guddat, ed., 1967, pp. 257, 253–54, 259, 261–62).

In 1843, then, Marx already stressed that the German bourgeoisie lacked its French counterpart's "revolutionary boldness," which "identifies itself, if only momentarily, with the soul of the people." Contrary to economic determinism, Marx argued that no "partial, political"—that is, bourgeois—revolution could take place in Germany. Only the famous radical union of philosophy and the proletariat would achieve human emancipation (communism). Here, Marx argued that not economic advance alone, but a special oppressiveness of early capitalism might lead to proletarian revolution.

Could Germany's "more developed" proletariat (in comparison to that which participated in the French Revolution) sustain a communist revolution? In the 1840s Marx often used the term "proletariat" to include such dispossessed artisans as the rebellious Silesian weavers as well as factory workers (Lubasz, 1976, p. 27). In *The German Ideology* (1966, pp. 73–74), Marx argued specifically that international textile competition from the most advanced capitalist power helped to forge an internal German proletariat and led to modern class conflict:

> To lead to collisions in a country, *this contradiction* between productive forces and the form of intercourse *need not necessarily come to a head in this particular country.* The competition with industrially more advanced countries, *brought about by the expansion of international intercourse,* is sufficient to produce a similar contradiction in countries with a backward industry (e.g., the latent proletariat in Germany brought into view by the competition of English industry).

By a "much more developed proletariat" Marx also referred to the emergence of capitalism in the Rhineland; to the increased experience of the European working-class movement, especially Chartism in England; to the Lyons strikes of the early 1830s; to the Silesian weavers' revolt; and to the development of socialist and communist theory, including Marx's own contribution, after the French Revolution. Marx's use of the term "proletariat" designated an important historical force in 1848. Yet the German proletariat had not been concentrated in large factories to nearly the same extent as the Russian proletariat of 1917, to say nothing of the advanced proletariat required by an economic determinist argument (Portal, 1965, pp. 852–59; Noyes, 1966, pp. 20–21). The skepticism of some commentators (Lichtheim, 1965, p. 89) about the existence of an "adequate" proletariat to achieve socialism indicates how strongly the economic determinist view of Marx holds them under its spell. In the 1840s, Marx himself held no such view.

Between 1845 and 1848, however, Marx studied the French Revolution more closely and concluded that the German proletariat would have to undergo a democratic revolution in order to win the masses of the population to a communist revolution. Only their own political experience could reveal the social limitations of formal political equality. In turn, abstention from this "real movement" would result in the utter political isolation of a socialist sect. Writing in the *Neue Rheinische Zeitung*, Marx reshaped his strategy for revolution in the light of French experience.

First, Marx saw the political foundation of the French Revolution as an alliance of the workers, including artisans and peasants, led—with reservations—by the bourgeoisie. In 1848 Marx bitterly criticized the bourgeois parliament at Frankfurt for opposing peasants' demands to abolish the remaining feudal services:

> The French bourgeoisie began with the emancipation of the peasants. With the peasants, it conquered Europe. The Prussian bourgeoisie has forfeited even this ally. . . (Padover, ed., 1971, pp. 474, 436).

Marx persuaded the Rhineland democratic congress to pass resolutions advocating an alliance of workers and peasants, and organized a rally of 10,000 workers and peasants in rural Worringen, near Cologne, on September 17, 1848. Beyond driving democracy forward, the newspaper of the Cologne Worker Society, led by Marx's colleagues Moll and Schapper, suggested, "The revolutionary power of Germany lies in the workers and peasants. . . . When the workers and peasants unite . . . then they will quickly free themselves from the burdens of feudalism and the usury and oppression of capital." The Worringen rally opposed any Prussian attempt to disarm the Cologne civic militia, and called for a "democratic socialist red republic." (Though the democratic revolution did not unfold strongly enough to make socialism a real possibility, Marx thought that communists could win the German peasantry, through its political experience, to support collective ownership as opposed to land division and impoverishment.) Contrary, then, to the economic determinists' stereotyped notion that Marx dismissed the peasantry as reactionary, the first communist rally which Marx initiated took place in a peasant community (Becker, 1963, p. 130; Hammen, 1972, p. 689; Marx and Engels, 1962, Vol. 1, p. 114).

In 1849, Marx hailed the French peasant and communist demand for the return of a billion francs which the restored monarchy had paid to the aristocrats in 1825 in compensation for their previous expropriation. Marx referred to this demand as the "first measure to draw the peasants into the revolution" and noted the emerging mass movement in Cluny, Agey, Ancey and other communes. Beyond this, through circulating *NRZ* articles in the Silesian countryside Marx and his co-editor Wilhelm Wolff instigated a Silesian peasant movement for return of a similar billion. An anguished aristocrat reported to the Prussian parliament that "the tale of the Silesian billion has affected the rural population of Silesia in an unhealthy manner" (Marx and Engels, 1959, Vol. 6, pp. 355–58; Hammen, 1972, p. 694). Thus, the *NRZ* contributed to the international interplay of French and German peasant radicalism. Marx's development as a theorist and organizer passed from observation of revolutionary movements (the French Revolution, the demand for the milliard) to the formulation of new theories and political conclusions (his strategy of the worker-peasant alliance in the democratic and then the socialist revolution) to subsequent action based on these conclusions (the Worringen rally, the Silesian campaign)—a movement from practice to theory to practice.

Second, Marx contrasted the English and French Revolutions, which "proclaimed the political system of the new European society," with the Prussian March Revolution, "a weak repercussion of a European revolution in a backward country, like the light from a long extinct star." As the German bourgeoisie "already belonged to the old society," the revolutionary masses, contrary to Marx's view in 1843, would provide the driving force in a democratic revolution. In mid-September, during the Schleswig-Holstein crisis, 8,000–10,000 boxing club members "morally" supported the "left" in the Berlin Assembly (Marx and Engels, 1972, pp. 183, 184). The conservative *Kölnische Zeitung* complained that the presence of the people—as opposed to the "normal" influence of Prussian bayonets and bribery—disturbed "freedom of deliberations." Relying on preceding revolutions, Marx responded:

> The right of the democratic popular masses to exert a moral effect on the attitude of constituent assemblies by their presence is an old revolutionary right of the people, which, since the English and French revolutions, cannot be dispensed with in *stormy times. To this right history is indebted for practically all the energetic steps taken by such assemblies* (Padover, ed., 1971, pp. 439, 440).

Popular pressure alone could force the new parliament to take "energetic" and "dictatorial" measures against the old feudal institutions. In September, Marx and Engels organized a rally of some 6,000 in Cologne, which set up a popular Committee of Public Safety modeled on the Jacobin Committee in the French Revolution. As it became clear that the revolutionary ground swell could not sustain such a measure, Marx and his followers dissolved the committee. Contrary to Lichtheim, Marx did not succumb to the magic of republican rhetoric, which he would criticize in the *Eighteenth Brumaire* (Marx and Engels, 1972, pp. 124, 126, 182, 187; 1962, Vol. 1, pp. 247–50). He sought only to use the practical lessons of the French Revolution to drive the German revolution forward as quickly as possible.

The increasingly obvious counter-revolutionary politics of the bourgeoisie during 1848 drove Marx to conclude that only workers and peasants led by communists could effectively carry out the bourgeois-democratic revolution in Germany. In 1849, he attempted to form a "purely social" working-class party to lead this movement (Marx and Engels, 1959, Vol. 6, p. 584). Contrary to economic determinist stereotypes, this conclusion prefigured the strategy of twentieth-century communist revolutions.

Third, Marx compared the contemporary international situation to that of the French Revolution. As the Duke of Brunswick's threat to raze Paris resulted in the convention's *levée en masse,* so counter-revolutionary intervention in 1848 would strengthen the democratic revolution. Democratic movements in Germany and Poland had already made allies of tsarist Russia—fearing an internal democratic revolution—

and capitalist England–trying to cope with Chartism, then at its peak (Marx and Engels, 1959, Vol. 5, p. 154; 1972, pp. 204–05; Fernbach, 1974, p. 150). These powers could count on support by the Prussian and Austrian monarchies. Since Prussia and Russia had partitioned Poland in 1772, a successful Polish nationalist movement would strike a blow at both absolutisms. Marx thought that counterrevolutionary unity of the most backward nations (Russia, Prussia), and the most advanced (England), standing opposed to strong democratic revolutions in Poland and Germany, would generate solidarity among the democratic movements and result in republican world war against absolutism. World war, in turn, would greatly strengthen the mobilization of the people and the ability of a protracted democratic revolution to root out the monarchy in Germany.

Consequently, Marx's strategy for democracy and proletarian revolution in 1848 rested on a specific international configuration of reactionary powers, and not on the development of productive forces (or on capitalist crisis) in a single country. Marx's argument foreshadowed Lenin's analysis of the role of imperialism and world war in creating a revolutionary situation in 1917.

In 1848 Marx and Engels also argued that Prussian efforts to crush Polish independence by reestablishing a patriotic army could defeat the German revolution; Engels referred to Prussia's "wars of restoration." Hence, Marx and Engels stressed the need for internationalism among the democratic movements and, unlike most German democratic leaders, fought consistently against Prussia's nationalist and racist campaigns against the Poles, Italians and other peoples. As Engels put it:

> A nation which throughout its history allowed itself to be used as a tool of oppression against all other nations must first of all . . . [proclaim] the freedom of the nations hitherto suppressed by it (Marx and Engels, 1959, Vol. 5, pp. 202, 84).

In the *Neue Rheinische Zeitung* Marx emphasized that only such democratic internationalism, as well as support for working-class movements like the Paris June insurrection, could defeat the monarchical counter-revolution which would otherwise divide and conquer.

Fourth, in the French Revolution radicals like Babeuf had discovered that despite the overthrow of the monarchy and the victory of political equality, social inequality still existed. Why, Marx asked, should armed and mobilized workers and peasants submit to continued capitalist oppression? In 1848 Marx stated graphically, "the republic [Second Republic, which overthrew Louis Philippe] bared the head of the monster [capital] itself when it knocked off its sheltering and secret crown." As long as the monarchy could serve as a "lightning conductor" for popular discontents, the bourgeoisie would probably lead the movement (Padover, ed., 1971, pp. 32, 148; Marx and Engels, 1972, p. 141). Once a working class might initiate an uprising or even a communist revolution. In the transition from a protracted democratic to a proletarian revolution Marx looked not to economic stages–depending on a new development of productive forces–but to political ones, resting on the revolutionary change from a monarchy to a capitalist republic.

These arguments taken together indicate that given the strengthening of some tendencies in the revolution, Marx had a realistic strategy for democracy and socialism in 1848. His argument is startling only in its inconsistency with the economic determinist view usually invoked in his name.

4. The Dialectic of Strategy and Explanation and the Worker-Peasant Alliance

Following a schema suggested by Hilary Putnam, Marx's arguments in 1848 might have looked something like this:

1. Guiding hypotheses and general theory as outlined in the first sections of the *Manifesto.*
2. Auxiliary Statements:
   (a) Due to English competition and the rise of German industry, a small proletariat existed in backward Germany;
   (b) A worker-peasant alliance, modeled on the French Revolution, would drive the German democratic revolution forward;
   (c) The German bourgeoisie, given its own weakness and the experience of France, would vacillate toward compromise with absolutism;
   (d) Strong democratic revolution in Germany or Poland would trigger world war with tsarist Russia and England, further strengthening the mobilization of the German people;
   (e) As in the French Revolution, a sufficiently strong democratic revolution could give rise to a communist movement;

   (f) Political forces existed in the German revolution—especially Marx and his followers—who sought to propel the movement in this direction.

3. Strategy [prediction] that the democratic revolution in Germany would immediately precede a proletarian revolution.

In the actual event of 1848, this strategy helped to forge a worker-peasant alliance. Yet the counter-revolution eventually triumphed. Faced with this defeat, Marx did not alter his basic theory. Instead, he advanced a number of new economic and political auxiliary statements to explain the result: the defeat of the June insurrection as a spur to counter-revolution; the revival from the trade and commercial crisis of the mid-1840s and the discovery of gold in California; and the lack of an independent working-class party (Padover, ed., 1971, p. 43; Fernbach, 1974, p. 275; Marx and Engels, 1962, Vol. 1, p. 106). The testing of strategy and discovery of its weaknesses in the actual event led to new explanations requiring different auxiliary statements.

Scholars have often ignored Marx's constant refinement of his theory based on new political experience. Marx's changing views on the role of the German bourgeoisie between 1843 and 1850 show how strongly such experience affected his strategic estimates. In 1843, he looked toward a radical social revolution which would overthrow the bourgeoisie along with the old order. Between 1845 and 1848, he envisioned the experience of a democratic revolution as a prelude to a proletarian one, and thought that the bourgeoisie must play some role—probably a vacillating one—in the fight for democracy. In 1848 itself, as part of the left wing of the democratic movement, he criticized the bourgeoisie for lack of energetic revolutionary measures. In later 1848–49, given the counter-revolutionary policies of the bourgeoisie, he called for "social-republican revolution," began to organize a "purely social" working-class party, and attacked the bourgeoisie as an exploiting class. In 1850, reassessing the revolutionary experience, he stressed the role of a proletarian party which would radicalize the democratic revolution and build for socialism independently of the bourgeoisie (Marx and Engels, 1972, pp. 31, 202, 206; 1962, Vol. 1, pp. 106, 111–12, 116). These changes in Marx's strategy underlined the role of a communist-led worker-peasant alliance in the struggle for both democracy and socialism.

Given the vicissitudes of class struggle, Marx's theory of capitalism could lead to varying political conclusions. Historical materialism provided a basic framework for evaluating fresh experience; one cannot understand the changes in Marx's position without attention to the underlying theory. But the reverse also holds: the general theory provided no excuse for ignoring novel historical circumstances or political events. In forging explanations or strategies, Marx's general theory set certain boundaries. At one extreme, without the existence of a proletariat and a mass movement, the political will of isolated conspirators would accomplish nothing. At the other, sheer reliance on material production, disregarding or even opposing revolutionary political uprisings, would fail. Between these two extremes Marx utilized his general analysis to work out the changing potentials of political movements within definite historical situations.

As Lenin argued in 1906, the implementation of strategy is precisely the realm of the unexpected:

> One cannot weigh with apothecary's scales the equilibrium between the new forces of counter-revolution and revolution which are growing and becoming interwoven in the [Russian] countryside. *Experience alone* will completely reveal this; revolution, in the narrow sense of the term is an acute struggle, and only in the course of the struggle and in its outcome is the real strength of all the interests, aspirations and potentialities displayed and fully revealed (1972, Vol. 11, p. 344).

Even under the most favorable circumstances, the elaboration of strategy requires as much of the great politician's skill as of sheer theoretical analysis. A specific revolutionary movement has no guarantees, just rough likelihoods of success. Here chance plays an important part. After the event, however, a better explanation becomes possible. In this respect, Hegel's owl still flies at dusk.

Marx's 1850–52 analysis of the role of peasant support for Louis Napoleon further illustrates the striking relationship between prediction and explanation. In *Class Struggles in France* (1850), written before Napoleon's victory, Marx traced the specific economic and political factors which continued to "revolutionize" the French peasantry. Despite the peasants' conservative attachment to small pieces of property, Marx argued, French capitalist development, through mortgages and usury, had reduced the peasants' standard of life to that of the famished Irish tenant. They had become, in a sense, workers. The new republic levied heavy taxes on the peasants while simultaneously introducing universal suffrage. It accentuated the contradiction between formal political equality and economic exploitation. As an admirer of Balzac's 1844 novel, *Les Paysans,* Marx's analysis echoed old Fourchon's debunking of ostensible peasant "freedom": "Liberated, are we? . . . The hoe, our only property, has not left our hands. Whether it is the *seigneur* or the tax collector, who takes the best part of our labors, we still have to spend our life in sweat" (Balzac, 1970, pp. 132–33). In contrast to the *Manifesto,* Marx now argued that capitalist undermining of smallholders would drive them not mainly to "hold back history" but to make common cause with urban workers. Marx predicted that a specific combination of economic and political forces, including socialist organizing in the French countryside, would overcome the peasants' conservative aspect and generate a strong socialist movement. In 1850, Engels' *Peasant War in Germany* (1966, pp. 74–76) emphasized even more strongly the role of radical ideology, a "proletarian-plebeian asceticism," in unifying otherwise scattered sixteenth century peasants in a general revolt.

Class Struggles identified a widespread "social democratic" movement among the French peasants which agitated for an uprising in the year "1852." As Roger Price, Albert Soboul and T. J. Clark have shown, radical peasants aimed to dispossess declining aristocrats and rising usurers and to reclaim what they regarded as rightfully their own. The procureur general at Aix complained, somewhat exaggeratedly, "There is not a peasant who does not regard the rich man or the bourgeoisie as the oppressor of the poor and who does not believe that the workers should be masters of France because they are the only producers." Popular songs rang out with social hatred:

> Misery has reappeared
> The harvest approaches and the peasants
> Will come to reap amongst the tyrants. . . .
> Misery produces a rage,
> And if they [the peasants] complain,
> they are cursed.
> Let them have cause to curse us!
> The end must come as soon as possible
> So that the bailiff will not seize it
> Carry the straw to the chateau.

In 1851, rural democratic secret societies mobilized large numbers of peasant fighters against Napoleon's coup, 4,000–5,000 in the Drôme and 15,000 in the Basses-Alpes. Agrarian capitalist exploitation had driven formerly stable poor peasants to desperation (in lower Languedoc, eight out of nine peasants deported after the coup were fathers of families). On trial, peasant witnesses still averred that "in 1852 property will be shared out" (Price, 1972, pp. 301–02, 305, 307, 295–96, 298–99).

Nonetheless, in 1851, Louis Napoleon triumphed with widespread rural electoral support. Many peasants who rebelled against the "republic of the rich," identified a restored Napoleon with the security of small property and patriotic victories won under his illustrious predecessor. Capitalist exploitation, Marx argued, had rendered these hopes "illusory." Still, some peasants even entertained "the grotesque notion of a [Jacobin] convention side by side with Napoleon" (Marx and Engels, 1962, Vol. 1, p. 336). In explaining Bonaparte's victory, Marx stressed that this political tradition of Napoleonic "hallucinations" had shifted peasant revolt against the republic to the right rather than the left. Even here Marx emphasized that upon becoming disillusioned with Napoleon,

> the French peasant will part with his belief in his small holding, the entire state edifice erected on this small holding will fall to the ground and the proletarian revolution will obtain that chorus without which its solo song becomes a swan song in all peasant countries (Marx and Engels, 1962, Vol. 1, p. 340).

The triumph of Napoleon flatly contradicted Marx's prediction in *Class Struggles* but did not refute his general theory. Instead, Marx explained it by changing an auxiliary statement and stressing the role of political survivals or traditions. Yet his new explanation had political implications beyond the particular case of France. Since only political activity, not economic development alone, would dissolve such traditions, Marx's new argument reinforced his strategic emphasis on revolutionary organizing among the peasants.

Lichtheim suggests that Marx gave up his worker-peasant alliance strategy—his alleged "Jacobinism"—after the triumph of the counter-revolution. But this argument triply obscures Marx's real position. It ignores Marx's initiation of a worker-peasant alliance in the democratic revolution in 1848; it overlooks Marx's strengthened conclusions about revolutionary activity in the countryside; and it dismisses Marx's continued stress after 1850 on this strategy in "all peasant countries." On April 10, 1856, Marx wrote to Engels that a "second edition of the Peasant War" must back a proletarian revolution in Germany (Marx and Engels, 1965, p. 92). In 1871, in language reminiscent of *Class Struggles* and the *Eighteenth Brumaire,* he analyzed the peasants' interests in defending the Paris Commune and stressed the bourgeoisie's fear of "the spread of the *rinderpest,*" that is, the revolution from the city to the countryside. Military encirclement deprived the Commune of the three months' free communication needed to inspire a "general rising of the peasants" (Marx, 1966, pp. 77, 175–76). In 1875 he chided the German Social Democrats' dismissal of the peasantry as part of "one reactionary mass" opposed to the revolutionary proletariat (Marx and Engels, 1962, Vol. 2, pp. 25–26, 39). In Marx's notes on Bakunin's *State and Anarchy,* he insisted that in continental Europe, a workers' government must serve the peasants at least as well as the French revolutionaries had done or the peasantry would "doom every workers' revolution." This proletarian government must secure the peasant's support for collective ownership "of his own accord on economic grounds." More than in the *Eighteenth Brumaire,* French defeats had taken their toll on Marx's optimism about the power of a sheer economic push toward socialist revolution. The peasants' "secondary exploitation" by the capitalist did not simply produce a proletarian response; instead "delusive prejudices" predominated: "even where [the peasant] does belong to [the proletariat] by reason of his position, he does not consider himself as belonging to it" (Marx and Engels, 1959, Vol. 18, pp. 632–33). Contrary to the German Social Democrats' Gotha Program, only persistent political efforts could win the peasants to translate their interests into action to achieve socialism. Thus, political experience continued to influence even the nuances of Marx's theory and strategy.

5. Chartism and Marx's New Theory in *Capital*

Before 1848 Marx worked out a second strategy for proletarian revolution based on a study of English Chartism. Engels, in his 1845 *Condition of the Working Class in England,* first outlined the main phases of this movement. The original and "least successful" stage of worker hostility to exploitation—theft—left the individual workers at the mercy of the government. The second, Luddite machine-smashing, the "first organized resistance of the workers," occurred in mutually isolated localities and industries, and resulted in severe punishment. The third—unionism—organized workers countrywide against the capitalists, at least in a given trade. Although "the history of trade unionism is the story of many defeats and of only a few isolated victories," Engels saw unions as a standing protest against the pressures of capitalism:

> The incredible frequency of strikes affords the best proof of the extent to which the social war now rages in England. Not a week passes ... without a strike occurring somewhere; ... these strikes are often nothing but skirmishes in a social war ... a training ground for the industrial proletariat and a preparation for the great campaign which draws inevitably nearer. Strikes are the manifestos by which particular groups of trade unionists pledge their adherence to the cause of the working class (Engels, 1968, pp. 243, 224, 246, 297, 259, 255–56).

This revolutionary interpretation of unionism distinguished Marx and Engels both from the utopians and from most union leaders. In an 1875 letter to Bebel, Engels pointed out that all French socialists in the 1840s opposed "combinations" (unions) and strikes, adding "with the exception of us two, who were unknown in France" (Marx and Engels, 1965, p. 300). In the *Manifesto,* then, Marx and Engels grasped the economic and political significance of the new working-class movement—the "real movement" as they called it. Socialist "sects" abstained from or opposed this movement and even English union leaders with whom Marx cooperated in the International Workingmen's Association opposed strikes and class war (Marx and Engels, 1962, Vol. 1, pp. 61–64). Engels' view of strikes as "skirmishes in a social war" laid the foundation for the distinctive Marxian concept of unionism.

The weakness of unionism, however, led to a fourth stage: a "purely political movement," such as Chartism, which could unite the workers in general as a party against their oppressors. The aims of such a party would include legislation for universal suffrage and shorter hours. Engels also foresaw a fifth stage, in which socialism—then a middle-class movement in England—would fuse with Chartism (1968, pp. 258–59, 261).

In *Poverty of Philosophy* and the *Manifesto,* Marx elaborated a strategy for a working-class movement which combined the last three stages of Engels' earlier account. Marx argued that workers formed unions as part of a "veritable civil war." To achieve their goals, unionized workers would have to engage in "political struggle" like the Chartists. Marx stressed the need to study this "real movement" of the working class, comparable to the "much researched" development of the bourgeoisie as a class:

> But when it is a question of making a precise study of strikes, combinations, and other forms in which the proletarians carry out before our eyes their organization as a class, some are seized with real fear and others display a transcendental disdain.

While Engels' *Condition,* still influenced by "true socialism," foresaw the role of communists as pacifying the workers, Marx viewed class struggle as the violent "shock of body against body." Only proletarian revolution could lead to fundamental social transformation: the abolition of classes (Marx, 1963, pp. 172–75).

In the *Manifesto,* Marx added that communists had a twofold political role within this working-class movement. In the "movement of the present" (the union struggle), communists, the "most advanced and resolute" group among the workers, should "push forward all the others." Left at this, however, communists would become reformers. Marx argued that communists must also link the current struggle and the "future" of the movement by defending internationalism—"the common interests of the entire proletariat independently of all nationality"—and urging the abolition of bourgeois property in the means of production (Marx and Engels, 1962, Vol. 1, pp. 46, 65). Advocacy of the future of the movement—abolition of exploitation—partly grew out of the movement of the present as a mass struggle against the capitalists for better working conditions, and yet partly conflicted with its goal of a "better" rate of exploitation from the workers' point of view. Contrary to narrow economic determinism (or later Russian "economism"), Marx dialectically stressed revolutionary political activity within the current union struggle.

In Germany in 1848 the union movement had barely begun. In Cologne, Marx could not initiate unionism and simultaneously lead the left wing of the democratic revolution; he chose the latter alternative. Nonetheless his followers won leadership in the large (7,000-member) Cologne Worker-Society and influenced it to play a role in the democratic revolution. After 1864, however, Marx implemented his strategy of radical unionism in the IWA. In becoming its main leader, Marx stressed international strike support and the fight for the shorter work week as well as support for the antislavery cause in the U.S. Civil War. He sought a broad internationalist framework encompassing union issues that would galvanize the most workers into participation. In the wave of European strikes between 1864 and 1868, the IWA vigorously supported the Leipzig compositers' strike, the English tailors' strikes of 1866 and 1867, the Paris bronze workers' strike, and others (Marx and Engels, 1962, Vol. 1, 373–89; Collins and Abramsky, 1965, pp. 61, 68–70, 82–83; Gilbert, 1978). As the IWA achieved broad influence among workers, some of the radical sects, such as the Lassalleans, reversed their position and supported unionism.

Despite the IWA's organizational dissolution in 1872, Marxian ideas achieved considerable influence in the eight hours movement in the United States in the 1880s and in the formation of a new socialist international (1889) closely linked to the union movement. Working-class commemoration of the U.S. shorter hours movement and the Haymarket martyrs on May Day, 1890, also revealed Marx's influence.

Yet Marx's revolutionary conception of unions distinguished him from other radical unionists. The IWA's internationalist strategy or the Bolsheviks' revolutionary activity in the unions differed markedly from the U.S. Communist party's militancy—without advocating socialism—in leading the sit-down strikes of the 1930s, or the pre-1914 German Social Democrats' submission to conservative union leaders (Prickett, 1975; Schorske, 1972, pp. 108–09; Krupskaya, 1930, pp. 139–40; Haimson, 1964). Such political differences, in Marx's view, would not arise from "inevitable" and spontaneous adjustments to circumstances. Rather, they represented political choices which could decisively affect the outcome of these movements.

While Marx and Engels advocated a strategy based on unionism in the 1840s, they held a general theory that wages must inevitably fall to a "bare existence," or a "wage minimum." But as Weston and others later argued, if unions and strikes could not gain permanent increases in real wages, why have them? (Marx and Engels, 1962, Vol. I, pp. 47–48, 88–89; Marx, 1963, p. 51). Let us examine this contradiction more closely. In *Condition* (1968, p. 246), Engels questioned the effectiveness of unionism:

> All these efforts on the part of trade unionists cannot change the economic law by which wages are fixed according to supply and demand in the labor market. Consequently, trade unions are helpless in the face of the major factors influencing the economy.

Engels stated that depressions (a major factor) forced unions to accept a reduction in wages, while in prosperity unions could force wages higher. From these true points, however, Engels concluded that unions "are not in a position to secure for their members higher wages than those which they would in any case obtain as a result of free competition between capitalists for skilled men." Again, one might ask, why have them?

But Engels then contradicted himself: "If the manufacturers did not have to face mass organized opposition from the workers, they would always increase their own profits by continually reducing wages." In other words, unions could significantly alter the level of wages. Engels also recognized that employers often used the argument that unions made no difference to undermine unionism while the workers themselves consistently adopted strikes and unions as a method of fighting (1968, pp. 246, 252). But, until Marx formulated his new theory in the later 1850s, they didn't resolve this confusion in their argument (1973, pp. 281–85; 321–25).

In our earlier terms, a basic principle of Marx's analysis of capitalism—the general theory that wages must decline to bare physical subsistence (Level 1)—contradicted his strategy based on unionism (Level 3). No auxiliary statement (Level 2) would extricate Marx from this difficulty. Which would Marx give up—the higher-level principle, or his analysis of the real movement? An economic determinist, having grasped the "basic" tendencies of capitalism, might argue that unionism should go. Marx, however, could resist neither the evidence of workers constantly striving to form unions, nor of the victory represented by the Ten Hours Bill in England. He gave up the inadequate principle.

Addressing the General Council of the IWA in 1865, Marx defended a radical conception of unionism against the Chartist, Weston. He argued that the worker got paid not for the "labor" of classical political economy, but for labor-power. Its value hovered around subsistence which Marx redefined to include both a physical and a very elastic "historical" or "social" element. Hence, economic and political organization could affect wages: "The fixation of its [the rate of surplus-value's] actual degree is only settled by the continuous struggle between capitalists and laborers." Contrary to economic determinism, the value of labor-power did not simply reflect an objective law "independent of man's will" but depended on political organization and varied with the tides of class struggle (Marx and Engels, 1962, Vol. 1, pp. 382, 442–43, 446).

Yet, one might ask, by this change in his theory, hadn't Marx just undercut his basic argument and justified gradualism? Marx regarded reforms under capitalism as limited and substantial wage increases as "exceptional." Over the long run, if unions drove up wages, capitalists would introduce machinery and lay off workers. As this tendency became sufficiently pronounced it would "progressively turn . . . the scale in favor of the capitalists against the working man." To defeat the capitalist class, workers must inscribe on their banner not the conservative slogan of "a fair day's pay for a fair day's work!" but the "revolutionary watchword," "abolition of the wages system!" (Marx, 1961, Vol. 1, pp. 612–712; General Council of IWA, n.d., Vol. 1, p. 272).

For Marx the practical anomaly between unionism and the iron law of wages focused attention on the contradiction within classical political economy between its definition of socially necessary *labor-time* as the measure of value and the value of "labor" as subsistence. It therefore contributed to what many have regarded as Marx's fundamental theoretical revolution in *Capital:* his conception of surplus value (Althusser, 1965, Vol. 2, pp. 116–26).

The effect of the union movement and strategy on Marx's theory of surplus value provides a valuable illustration of the process by which Marx refined or, in this case, revolutionized his theory: a movement from an older, somewhat contradictory theory (wages will decline to bare subsistence *and* unionism) through practice (continued development of unions and strikes) to Marx's new theory in *Capital.* This process grew out of the one we traced earlier: practice (Chartism) to theory (the *Manifesto*'s strategy for communist activity) to practice (International Workingmen's Association). This overall pattern—developing arguments from revolutionary experience and applying them, evaluating the new experience and further elaborating the theory—parallels the dialectic of strategy and explanation which we found in Marx's views of the peasants. Yet this anomaly between unionism and the "iron law" exerted a far more profound effect on Marx's general theory than the unexpected triumph of the second Napoleon. In the case of the peasants, Marx changed auxiliary statements to explain Napoleon's victory. The later development in *Capital* reflected a change in the fundamental theory itself.

6. The Political Impact of *Capital*

Many scholars see *Capital* as one of Marx's most determinist, apolitical works, an analysis of "objective economic forces" which will automatically give rise to socialism. This notion, based on a misinterpretation of Marx's phrase about "natural-historical succession" of modes of production, misses the political force of Marx's argument.

The first volume of *Capital* played a major role in Marx's struggle within the IWA. The debate in the General Council, which drove Marx to write *Wages, Price and Profit,* preceded a similar argument with the Proudhonists at the 1866 Geneva Congress. In a resolution drafted by Marx on "the trade unions: their past, present, and future," he envisioned their historic role:

> Unconsciously to themselves, the Trades' Unions were forming *centers of organization* of the working class, as the medieval municipalities and communes did for the middle class. If the Trades' Unions are required for the guerrilla fights between capital and labor, they are still more important as *organized agencies for superseding the very system of wages labor and capital rule* (General Council of the IWA, n.d., Vol. 1, p. 348).

This resolution flowed not just from Marx's argument on subsistence in *Capital,* but also from Marx's theory of the general law of capitalist accumulation. Through the introduction of machinery, Marx argued, capitalism created a reserve army of the unemployed. To combat this tendency Marx sought to unite the more organized workers with the unorganized, particularly agricultural workers, as well as the unemployed:

> Apart from their original purpose, they [the unions] must now learn to act deliberately as organizing centers of the working class in the broad interest of its *complete emancipation;* ... they cannot fail to enlist the non-society men into their ranks. They must look carefully after the interests of the worst paid trades, such as the agricultural laborers, rendered powerless by exceptional circumstances. They must convince the world at large that their efforts, far from being narrow and selfish, aim at the emancipation of the downtrodden millions (General Council of IWA, n.d., Vol. 1, p. 349).

Marx also argued in *Capital* (1961, Vol. 1, p. 640) that wherever the unions "try to organize a regular cooperation between employed and unemployed, capital and its sycophant, Political Economy, cry out at the infringement of the 'eternal' and so to say 'sacred' law of supply and demand."

The argument in *Capital* reinforced Marx's fight for other causes in the IWA. In the "Inaugural Address" and in a resolution to the Geneva Congress, Marx stressed the limitation of the working day to eight hours as a "preliminary condition, without which all further attempts at improvement and emancipation must prove abortive." Such legislation, he argued, would maintain the health of the workers and secure time for intellectual development, social intercourse, and political action. *Capital* traced in detail the history of the struggle over the length of the working day. It stressed the Chartist movement for the Ten Hours Bill and even used a dialogue between a worker and a capitalist modified from a leaflet that London builders had issued during their strike for a nine-hour day in 1860.

> Suddenly the voice of the laborer, which had been stifled in the storm and stress of the process of production rises. "By an unlimited extension of the work day, you may in one day use up a quantity of labor-power greater than I can restore in three. What you gain in labor I lose in substance. I demand therefore a working day of normal length, and I demand it without any appeal to your heart for in money matters sentiment is out of place. You may be a model citizen, perhaps a member of the Society for the Prevention of Cruelty to Animals, and in the Odour of Sanctity to boot [Sir M. Peto, one of the construction owners, belonged to the Odour of Sanctity], but the thing that you represent face to face with me has no heart in its breast. I demand the normal working-day because I, like every other seller, demand the value of my commodity" (1961, Vol. 1, pp. 233–34).

The shorter hours movement among English workers and the reports of the factory inspectors strongly influenced the writing of *Capital; Capital,* in turn, sought to spur on the movement.

In *Capital,* Marx showed how capitalism, when not restrained by the power of the workers, displayed a "werewolf hunger for surplus labor" and prematurely wasted the workers' lives.

> It steals the time required for the consumption of fresh air and sunlight. It higgles over a meal-time, incorporating it where possible with the process of production itself, so that food is given to the laborer as a mere means of production, as coal is supplied to the boiler, grease and oil to the machinery.... It extends the laborer's time of production during a given period by shortening his actual lifetime (Marx, 1961, Vol. 1, p. 264).

Workers could stop this "vampire" quality of capitalism only by force: "Between equal rights force decides." Marx argued that "the creation of a normal working day is . . . the product of a protracted civil war, more or less dissembled, between the capitalist class and the working class." As the struggle for shorter hours "takes place in the area of modern industry, it first breaks out in the home of that industry—England." These English workers championed the cause of workers under capitalism everywhere (Marx, 1961, Vol. I, pp. 233, 235, 299, 302). In the United States, Marx linked the death of slavery in 1865 to a new life, the unified movement for the eight-hour day, initiated by the 1866 Baltimore Congress of Labor. In this context, *Capital* (1961, Vol. 1, p. 301) advocated an internationalist defense of the most oppressed workers in the struggle for shorter hours: "Labor cannot emancipate itself in the white skin where in the black it is branded." Nowhere more forcefully than in the chapter on the working day did the arguments of *Capital* flow from the political struggle of the international working class and return to it.

The British Parliament's Children's Employment Commission had restricted the use of child labor in metal and "home work" (putting out) manufacturing. In 1867, these manufacturers called for a new parliamentary investigation of factory conditions and an interim postponement of further supervision, allowing, as Marx put it, a fresh "five years' term of exploitation" of children. Some of *Capital*'s fiercest pages depicted this exploitation in pottery, matchmaking and the lace trade. Marx cited the 1860 report of Mr. Broughton Charlton, a county magistrate in Nottingham:

> Children of nine or ten are dragged from their squalid beds at two, three or four o'clock in the morning and compelled to work for a bare subsistence until ten, eleven or twelve at night, their limbs wasting away, their frames dwindling, their faces whitening and their humanity absolutely sinking into a stone-like torpor (1961, Vol. 1, pp. 243–48).

In 1867, Marx encouraged action by the union leaders in the General Council to oppose any relaxing of the already weak child labor laws. In June, he wrote to Engels, "Fortunately my position in the International enables me to upset the tricky calculations of these curs [the manufacturers]. The thing is of the utmost importance. It is a question of *abolishing the torture* of one and a half million human beings." On August 15, 1867, Parliament limited the working day for women and children under 18 to ten and a half hours not only in large factories but in small enterprises and domestic industry (Marx, 1965, pp. 188, 510).

The section on machinery and modern industry in *Capital* also provided the theoretical basis for a General Council resolution to the 1868 Brussels Congress. Marx (1961, Vol. 1, pp. 435–37) noted that capitalists used machinery as a weapon directed against striking workers:

> Machinery not only acts as the competitor who gets the better of the workman, and is constantly on the point of making him superfluous. It is also a power inimical to him and as such *Capital* proclaims it from the roof tops. . . . It is the most powerful weapon for repressing strikes, those periodical revolts of the working class against the autocracy of capital.

Whenever workers gained shorter hours, capitalists would introduce machines to intensify their labor and spur productivity. Therefore, Marx suggested, so long as capital "convert[s] every improvement in machinery into a more perfect means of exhausting the workman," workers would renew their effort to reduce the hours of labor (1961, Vol. 1, p. 417).

Finally, *Capital*'s analysis of the general trends in capitalism strengthened the new socialist movement gradually developing within the IWA. The 1868 Brussels Congress hailed the publication of *Capital* directly. The arguments in *Capital* gave deeper force to IWA resolutions on the need for a working class party to conquer political power. These arguments also laid the groundwork for the IWA's international support of the Commune as the "political form at last discovered in which to work out the economic emancipation of labor" (Marx, 1966, p. 72).

A comparison of volume 1 of *Capital* with the relatively abstract discussion of commodities and money in *Contribution to a Critique of Political Economy* or the more extensive but equally abstract *Grundrisse*—both products of the late 1850s—demonstrates how dramatically the rebirth of the working-class movement in the 1860s and its political needs influenced the writing of *Capital*. Echoing occasional complaints in Marx's letters, many scholars have argued that Marx's activity in the IWA hindered the development of his theory. According to David McLellan (1973, p. 360),

> One of the main reasons why Volume One of *Capital* was so long in appearing and why the subsequent volumes never appeared at all is that Marx's time was taken up by the work forced on him as the leading figure in the International.

This view neglects the impetus to finish and publish *Capital* provided by the resurgence of the working-class movement. Marx spent the 12 years (1852–64) which preceded his activity in the IWA studying in the British Museum and writing newspaper articles. In that time, Marx produced the rough drafts for *Capital* and the minor *Contribution to a Critique of Political Economy*. From 1872 until 1883—another period of lessened political activity—Marx worked at but never finished the later volumes. A relatively calm, scholarly atmosphere did not inspire the completion of *Capital*.

Marx's overall political strategy for revolution consisted primarily of the radical union movement and the worker-peasant alliance. Marx combined these two for Germany in 1856. Three other points completed Marx's perspective on revolution.

First, within the union movement, or any movement of the present, communists would advocate internationalism, the abolition of bourgeois property, and after 1872, the introduction of a new kind of government modeled on the Paris Commune (Marx and Engels, 1962, Vol. 1, p. 22).

Second, a revolutionary movement or party should consist mainly of workers. Marx and Engels particularly insisted on this point in their Circular Letter of 1879 attacking the "three Zurichers." These three Zurich officials of the German Social Democrats had demanded that the party consist mainly of "independent representatives of science and all men imbued with true love of humanity" rather than workers. In reply, Marx and Engels stated (1965, pp. 321, 327):

> For almost forty years we have stressed . . . the class struggle between bourgeoisie and proletariat as the great lever of the modern social revolution. . . . When the International was formed, we expressly formulated the battle-cry: the emancipation of the working classes must be conquered by the working classes themselves. We cannot therefore cooperate with people who openly state that the workers are too uneducated to emancipate themselves and must be freed from above by philanthropic big bourgeois and petty bourgeois.

Third, the success of revolution depended on the emergence of a revolutionary situation. The three Zurichers had argued that German Social Democracy should follow a legal electoral road to socialism and oppose mass revolutionary violence. Marx and Engels (1965, pp. 323, 326, 327) responded sarcastically:

> So if the 500,000 to 600,000 Social Democratic voters—between a tenth and an eighth of the whole electorate and, besides, dispersed over the length and breadth of the land—have the sense not to run their heads against a wall and to attempt a "bloody" revolution of one against ten, this proves that they forever *renounce* taking advantage of some tremendous external event, a sudden revolutionary upsurge arising from it, or even a *victory* of the people gained from it. If Berlin should ever again be so uneducated as to have another March 18 [the beginning of the revolution of 1848], the Social Democrats, instead of taking part in the fight as "riff-raff with a mania for barricades" must rather "follow the path of legality," put on the brakes, clear away the barricades and, if necessary, march with the glorious army against the one-sided, coarse, uneducated masses. Or if the gentlemen assert that this is not what they meant, what then did they mean?

Unlike the Zurichers, Marx regarded legality as a matter of tactics, not strategy. With the unfolding of a revolutionary situation—spurred by a "tremendous external event" like war or economic crisis—the socialists might lead a successful uprising. This argument again underlined the importance of the international setting in Marx's thinking. A "victory of the people" in a "sudden upsurge" implied at least majority participation or sympathy in the whole population, so that the one-eight would no longer be isolated.

7. Politics and Socialism

Many scholars have overlooked Marx's political activity, his revolutionary strategies and the complex way in which they flow from his general theory. Yet Marx's attentiveness to history and to new political movements provides an important arena for evaluating the character of historical materialism as a social theory. Challenged about the alleged reductionism of this theory in the 1890s, Engels stressed that he and Marx—facing earlier opponents who ignored the role of modes of production—had sometimes overemphasized this economic foundation in their most general statements of the theory. But "when it came to presenting a section of history, that is, to applying the theory to practice, it was a different matter and there no error was permissible." Engels persistently referred to Marx's *Eighteenth Brumaire* as a model application of the theory, and an illustration of the "dialectics" of modes of production and political action, ruled out by crude economic determinism (Marx and Engels, 1975, pp. 396, 401–02, 443).

Marx's use of auxiliary statements in formulating strategies and his subsequent political refinements presented no immediate difficulty for his general theory. If an ultimate tension exists between Marx's two types of theorizing, however, it lies in the degree to which economic conflicts directly engender mortal revolutionary combat. Once again, a particular exception to the general path of development, say, a socialist revolution in Germany in 1848, or in Russia in 1917, need not throw the dynamic of Marx's pattern into question. As Lenin argued in 1923 against the Menshevik Sukhanov, "Where, *in what books,* have you [Sukhanov] read that such variations on the *customary* historical order of events are impermissible or impossible?" (1960, Vol. 3, p. 882). But a persistent failure of revolution in the more industrialized capitalist societies, despite a considerable degree of oppression, must lead to questions within Marx's own framework about the straightforwardly determining role of economic contradictions.

Oppression, often violent class conflict over unions, the formation of significant socialist and communist parties and the emergence of three international workers' movements have marked European and American history in the nineteenth and twentieth centuries. In Putnam's terms, the existence of such movements constitutes a striking predictive success for Marx's theory of capitalism's internal contradictions. Such movements show that capitalism creates a potential for working-class socialism. But proletarian revolution turned out, even in Marx's lifetime, to be a far trickier political proposition than Marx or Engels envisioned. Taken as a whole, Marx's constant refinements of explanation and strategy, especially on the role of English colonialism in Ireland and the peasants, accentuate the importance of political choices or alternatives, and would seem to require modification of the general theory in a more overtly political direction. Oppression, war and even large radical movements do not lead "inevitably" to the overthrow of capitalism.

In the twentieth century, socialist and communist movements have taken up some aspects of Marx's theory and strategy and discarded others. Since scholars have poorly understood the strategies and the theory, we may also misunderstand these adaptations in differing circumstances. Marx's strategies themselves exhibit a constant tension between the present of the movement and its future, between furthering a movement for reform and a protracted appeal to workers, peasants and intellectuals on a more revolutionary and internationalist basis. In this context, misuse of Marx's general theory may often have curtailed the revolutionary aspect of modern socialism and communism. Furthermore, Marx's own corrective, his willingness to learn from new political experiences, required great innovative skill in applying the theory in due time within serious movements.

Thus, the tensions between Marx's two types of theorizing and within his strategies themselves may have generated all sorts of problems for radical movements both before and after proletarian revolution. Following Hegel and seeking to criticize these experiences internally, we might restore a decisive aspect of the dialectic between politics or strategy and social forces, rubbed away until recently by economic determinist misinterpretation.

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COMMUNICATIONS

Comment on Rosenstone and Wolfinger (Vol. 72, March 1978, pp. 22–45)

TO THE EDITOR:

In the March 1978 issue of the *Review*, Steven J. Rosenstone and Raymond E. Wolfinger assert that the liberalization of selected state procedures for voter registration would have increased turnout in the 1972 election by 9.1 percent of the potential electorate. If this is so, then the total assumption by the government of the responsibility for registering eligible voters, or the elimination of prior registration for voting altogether would increase participation even more. The claim that additional election law reforms would have such a substantial impact on voter participation is important because of the widespread civic convern with declining voter turnout and persistent political demands for liberalized election laws to increase participation. The estimates of the impact of selected reforms offered by Rosenstone and Wolfinger are consistent with scholarly literature emphasizing that registration is the biggest barrier to voting in the United States (see Kelley et al., 1967; Merriam and Gosnell, 1924). Their estimates of the impact of election laws on voting rates using inferential statistics rests on an impressive data set: the massive 1972 census survey of registration and voting, with more than 90,000 respondents, combined with selected state-level election variables.[1] A reassessment of the evidence, however, suggests that Rosenstone and Wolfinger have greatly overestimated the ability of their proposed election reforms to increase voter participation.

Rosenstone and Wolfinger disregard information about the disinclination of much of the public to register and vote regardless of ease of access to the voter rolls. The 1972 census survey, upon which the authors rely for measures of voting participation, also asks the reasons for non-voting. The non-voting respondents were asked if they were registered to vote, and the unregistered were asked for reasons why they were not enrolled to vote. In a footnote to their study, Rosenstone and Wolfinger explain that they found respondents' testimony on reasons for non-registration unsatisfactory because no apparent relationship existed between the "objective" permissiveness of the state registration laws and the proportions of respondents who said that they could not satisfy residency requirements or had otherwise been unable to register. This information was discarded with the comment that the responses were "disillusioning for anyone who believes that human motivation can be discerned by asking people to explain their behavior" (Rosenstone and Wolfinger, 1978, p. 25, n. 17). But it is just possible that the authors' cross-sectional approach led them astray and that respondents perceived reasonably well that certain variations in state registration requirements did not have substantial effects on the public's ability to vote in 1972. The Voter Rights Act Amendments passed by Congress in 1970 forbade residency requirements of more than 30 days for presidential elections and required provision for absentee voting for those who had recently moved from their former state of residence. Prior to the 1972 general election, the Supreme Court struck down long residency requirements for elections below the presidential level also.[2] Thus residency requirements at least should not have had an appreciable effect on voting in 1972. My own longitudinal analysis of the impact of reduced residency requirements on the opportunity to vote suggests that reported inability to meet such qualifications is reasonably related to alterations in residency requirements throughout the states. For example, when the census survey first asked in 1966, about 6.0 percent of the responding public reported that residency prevented them from registering to vote, but in 1974 after the requirements were liberalized only about 1.6 percent reported residency as a barrier (Bureau of the Census, 1966, 1974).

The public's direct testimony about the reasons for its behavior should not be dismissed, but rather should be joined with other available information on registration and voting for a comprehensive analysis of the stimulating and restraining forces on voter participation. Obviously, a portion of the public reporting that restrictive procedures impeded their path to the polls might not have registered, much less voted, in the absence of those impediments. Nevertheless, the responses do suggest the size of the pool of potential voters affected by legal requirements. It seems appropriate, therefore, to compare Rosenstone's and Wolfinger's predictions about the impact of eased registration requirements on voting with the reported reasons people gave for not being registered. Table 1 gives the proportion of new voters whom Rosenstone and Wolfinger project would be added to the actual voters given both combined and separate liberalization of each of four registration law provisions. The four reforms that they recommend as likely to significantly increase vote turnout are elimination of days when the registration polls are closed before an election, provision of regular business hours for registration, provision of extra hours for registration, and provision for some absentee registration. Enactment of all four reforms together, they say, would increase voter turnout 9.1 percent. Table 2 gives the proportion of the public reporting various reasons for not registering. The reasons include alien status, residency requirements not met, inability to register, lack of interest in or dislike for politics and elections, or some other reasons. The inability-to-register category covered such reasons as lack of transportation, inconvenience of registration, ignorance of registration places or procedures, and illness or physical disabilities. The residual category of other reasons covered such vague responses as "hadn't bothered" or "haven't gotten around to it."

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[1] The authors added their state-level variable codings to the individual-level data with state identifications gathered by the census bureau and made available through the Inter-University Consortium for Political and Social Research.

[2] The 1970 Voting Rights Act Amendments imposed a maximum 30-day residency requirement for presidential elections. The Supreme Court, in striking down Tennessee's one-year residency law, suggested that non-presidential elections were also subject to a 30-day residency limit. See *Dunn* v. *Blumstein,* 405 U.S. 330 (1972).

Comparing the direct testimony on reasons for not registering with the statistical inferences about the impact on voting of easing registration requirements allows us to reassess the analysis of Rosenstone and Wolfinger. First, it should be noted that it would require an expansion of the reported registered voters by 10.5 percent to generate an increase in voting of 9.1 percent, even generously assuming that the enlarged registered public would vote at the same 87 percent rate reported for the actual registered respondents in 1972. The most likely recruits to the electorate under liberalized registration laws are those reporting inability to vote under the prevailing procedures. Even combining those so reporting with those unable to meet residency requirements yields only 5.1 percent of the public as potential enrolled voters. Thus the increase projected by Rosenstone and Wolfinger would also have to draw from the 4.2 percent of the public who found no specific obstacle to registration but still referred to it vaguely as a bother involving problems that they did not overcome. If registration requirements were eased, citizens from this group might be drawn into the electorate, but any inconvenience at all associated with registration would likely keep an appreciable portion of these people from bothering to register. Moreover, even if registered, members of this group would seem less likely to vote than those who actually voted under the

Table 1. Rosenstone and Wolfinger's Estimated Percentage Increases in National Voter Turnout from Combined and Individual Impact of Four Registration Reforms

Total Increase from Four Reforms	Non-Closure of Voter Rolls	Regular Office Hours for Registration	Evening and/or Saturday Registration	Some Absentee Registration
9.1	6.1	0.4	2.5	0.5

Source: Based on Rosenstone and Wolfinger, *American Political Science Review* 72:34, Table 4. The authors explain that the increases projected for each particular reform assume no changes in the other provisions. Thus the joint effect of all four reforms is not the sum of the four individual effects.

Table 2. Percentages of Voter-Aged Public Reporting Various Reasons for Not Registering

Alien Status	Residence Requirement Not Satisfied	Inability to Register	Dislike of Politics or Lack of Interest in Voting	Other Reasons
3.0	1.7	3.4	16.0	4.2

Source: Based on responses to the November, 1972 Current Population Survey of voter-aged civilian noninstitutionalized population conducted by the Bureau of the Census.

prevailing conditions. Thus, the increased turnout projected by Rosenstone and Wolfinger would depend on recruiting as voters some of those who do not register because of expressed dislike of or lack of interest in politics or elections. If the reports of the proportions of the population not registering for the various reasons are reasonably accurate, then it seems unlikely that merely easing the burden of registration would produce the increase in voting claimed by Rosenstone and Wolfinger. Moreover, the authors assert that their projections are based on assuming "relatively minor changes" in registration laws so that eliminating the requirement that citizens get themselves registered would increase voter turnout by "substantially more than 9 percent" (Rosenstone and Wolfinger, 1978, p. 41). Thus, either the information about the reasons for non-registration are incorrect or the projected effects of liberalized access to the voter rolls are inflated.

A preliminary reassessment of the Rosenstone-Wolfinger analysis suggests that their estimates may be inflated because political environment crucial to voter participation is also associated with the liberality of election laws. Most of the increased voting that the authors expect from legal reforms is based on the statistical correlation of high turnout with residence in states with relatively few days in which registration books are closed. Controlling for selected other variables, Rosenstone and Wolfinger infer that eliminating the days of closed registration rolls in every state would increase turnout by 6.1 percent. For a preliminary reassessment, this relationship can be displayed by taking advantage of the dependence of statistical inference on deviant or outlying cases from some norm or mean value. State codings provided by Rosenstone and Wolfinger indicate that 30 of the 49 states with voter registration in 1972 had closed registration periods of about three or four weeks. A few states closed their registration rolls much earlier than the norm, and several states closed their rolls much later than the norm. Rosenstone and Wolfinger treat these state laws as causal forces on voter turnout, but the outlying cases would provide the preponderance of evidence for the statistical inferences. Table 3 identifies the outlying states grouped on the closing date variable as identified by Rosenstone and Wolfinger so that the reader can get some sense of which state turnout rates are apparently demonstrating the alleged causal impact of the closing date variable. The mean state turnout for each group of states based on the official returns indicates an obvious simple association between closing dates and voter participation. But a perusal of the states in the outlying groups should suggest that the closing date groupings are associated with other state attributes. The low outliers with short periods of closed registration tend to have small populations composed of generally small communities. It is possible that such communities have stronger mutual expectations of the performance of civic duties than obtains in large metropolitan areas. There is no ready way of quantifying community orientations toward participation, but the simple measure of mean percent of state populations residing in metropolitan areas and central cities indicates that community type could easily be a confounding variable in the statistical relation between voting and state laws. The association of these measures related to community size and the length of closed registration rolls is obvious in Table 3.

Rosenstone and Wolfinger's inferential equation does not adequately control for the possible confounding effects of different political environments in the association between registration laws and voter participation.[3] Their only control for community type is a dummy variable for the South defined as the eleven former confederate states. Because the political environment of the South has traditionally depressed political participation, this is an important control. But the North-South dichotomization fails to distinguish among the southern states according to their political environments beyond election laws (Tennessee and the outer South is very different from Mississippi and the Deep South). The controls for the demographic variables of age and education (and race entered in their preliminary analysis) cannot control for different political environments outside the South. There is no control for community size which seems strongly related to both high turnout and liberal laws. Thus, the Rosenstone-Wolfinger estimates of the impact of registration laws must be treated with great caution because they are not compatible with other information in their data set and political environment is a credible rival explanation for the statistical association upon which the authors stake their claim.

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[3] The predictor variables in Rosenstone and Wolfinger's final equation for estimating the effects on voting of registration laws include the four registration law variables, the number of hours the polls were open, whether there was a concurrent gubernatorial election, and the demographic variables of age, education, and whether residence was in the South. Race and concurrent senatorial elections were found as having no independent association and were not included in the final equation.

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Reply

TO THE EDITOR:

Carleton Sterling's first point is that we should have paid more attention to respondents' expressed reasons for not registering to vote. He does not quite say that these explanations should be taken at face value, but neither does he suggest an appropriate discount rate. The closest he comes to a specific proposal is a recommendation that these excuses be "joined with other available information . . . for a comprehensive analysis."

Such analysis would require valid measures of the restrictiveness of the relevant state laws for each respondent in the sample. But the Census Bureau solicited the "reason you did not register" only from people who were not registered. There is no measure of the severity of the laws for those who did register. Thus one

Table 3. Voter Turnout and Population Concentrations for States Grouped by Periods of Closed Registration*

	States with Registration					
Closed Rolls	Less than 1 week (3 days)	About 1 week (8–11 days)	About 2 weeks (13–17 days)	About 3–4 weeks (20–32 days)	About 6 weeks (38–42 days)	About 7 weeks (50 days)
States	Idaho Oregon Vermont	Alabama Iowa Maine Nebraska Oklahoma Utah	Delaware Ohio South Dakota Wyoming	Most states	Nevada New Jersey New Mexico	Arizona Georgia
Percent Turnout of Voter-Aged Population	62.0	59.0	63.1	54.9	56.2	43.0
Percent Metropolitan of State Residents	25.6	44.0	40.6	57.5	62.9	61.5
Percent Central City of State Residents	11.9	24.9	14.5	27.2	27.0	35.0

Source: The voter turnout is based on officially tabulated votes for president in 1972 as a percentage of voter-aged population as reported in the Bureau of the Census, *Statistical Abstracts of the United States,* 1977, p. 511, Table 819. The metropolitan and central city percentages are based on state percentages reported in Michael Barone et al., *Almanac of American Politics, 1972.* North Dakota has no registration and is excluded from this analysis.

*The values are mean state percentages for each group of states.

cannot even construct a simple 2 x 2 table of turnout by severity of the laws and standard statistical methods of estimation could not be used. It is difficult to see what sort of "comprehensive analysis" would be possible in such circumstances.

More important, how valid are the answers to this Census Bureau question? Do they accurately measure the severity of the registration provisions in each state? As we noted, we found no relationship in 1972 between the actual restrictiveness of a given state's laws and the likelihood that respondents would cite the laws as reasons for not registering. In fact, Converse and Niemi (1971, p. 456) report that some respondents who said they "could not register" lived in places where voter registration was not required.

Sterling contends that the reasons an individual gives for not registering are a valid indicator because the propensity of unregistered persons to blame residency requirements declined between 1966 and 1974, a period during which the requirements were liberalized. Sterling fails to report that the Census Bureau asked very different questions in 1966 and 1974. The 1974 question was this simple open-ended item: "What was the *main* reason (this person) was not registered to vote?" (Bureau of the Census, 1976, p. 95.) In 1966, on the other hand, respondents were prompted:

> Two specific reasons were mentioned in the question as it was read by the interviewer, i.e., that the respondent was not a citizen or had not met the local or State residence requirements. In addition, the respondent was invited to give another reason if neither of these adequately explained his failure to register (Bureau of the Census, 1968, p. 3).

It is not surprising that this wording elicited more references to residency requirements than the nondirective question used in 1974. Moreover, the pattern of the 1966 answers led to doubts about their validity in the Census Bureau report:

> Failure to meet residence requirements was cited by only 7 percent of those persons with less than 8 years of schooling, but was the most common reason for not registering (42 percent) given by respondents who were college graduates. This may well reflect the fact that people with little education are not aware of the residence laws for voting or do not know what they require, and, therefore, are more accurately categorized as apathetic. College graduates, on the other hand, may be tempted to give residence requirements as a reason, rather than reporting a less acceptable reason (Bureau of the Census, 1968, p. 3).

In view of the shaky validity of respondents' justifications for not registering, we preferred to estimate the impact of the actual statutes on the likelihood of voting.

Sterling's second point is that our findings were inflated because states with high turnout and lenient closing dates also "tend to have small populations composed of generally small communities. It is possible that such communities have stronger mutual expectations of the performance of civic duties than pertains in large metropolitan areas."

With other things held constant, are people living in "small communities" more likely to vote than those in metropolitan areas? In a later paper based on the same data set we reported finding no difference in turnout between farm residents and people in metropolitan areas (Wolfinger and Rosenstone, 1977). The only difference we did find was slightly *lower* turnout—one to three percent—by rural residents who did not live on farms. With the inclusion of this variable, the coefficients for the registration statutes all remain within a standard error of those reported in our *Review* article. In short, the effect of community size is small, exists only for rural non-farm residents, and is in the opposite direction from that suggested by Sterling.

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Comment on Kernell
(Vol. 72, June 1978, pp. 506–22)

TO THE EDITOR:

Samuel Kernell's article, "Explaining Presidential Popularity" (1978) is the latest in a lengthening series of works of research attempting to explain trends in presidential popularity. This body of literature, following John Mueller's lead, uses the population of Gallup's presidential popularity items as a data base. Previously mentioned problems connected with such research include under-representation of certain groups in the earlier quota sample polls (Glenn, 1970), unsystematic use of filter questions (Glenn, 1970), and statistical problems with autocorrelation and feedback effects (Hibbs, 1974). Kernell's analysis recognizes these problems.

There are, however, additional difficulties which need to be articulated. These difficulties include response set bias, sampling misconceptions and temporal considerations. We feel these difficulties are important enough to raise questions about substantive conclusions forthcoming from research ignoring them. This is especially true for research such as Kernell's which attempts to attribute variance in presidential popularity to discrete events.

Respecting response set bias, users of the Gallup presidential popularity item (PPI) rely implicitly on the independent nature of the respondent's judgment. If, however, questionnaire items administered before the PPI focus respondents' attention on relevant aspects of presidential performance or on "better" alternatives to the administration itself, then those prior items are likely to bias respondents' reactions to the later PPI.

Before 1956, the PPI typically is buried in the questionnaire. Since 1956, except for three periods during Johnson's administration and occasional other exceptions, it is the first item on the questionnaire. The periods in the Johnson administration in which it occurs later in the interview are May 1965 to May 1966, September 1966 to December 1966, and November 1967 to April 1968. Regarding these periods, Stone and Brody comment that "opinion polls during the last three years of the Johnson administration showed massive shifts in the proportions approving how the President was handling his job. . . . Rarely has the public been so responsive. . ." (1970, p. 95).

To what are respondents responding? Consider poll 756, administered in January 1968 while Johnson still was considered a candidate for reelection. This poll is typical of those administered during the periods in question. In this case, the presidential approval item is question 17. Before it was asked, respondents on at least six separate occasions are asked to make a choice between the incumbent President Johnson and the best of the two major parties' contenders for the presidential nomination. They are reminded that the Vietnam War is likely to "last." The specter of price and wage freezes is raised (Gallup, 1972, pp. 2101–07). Only after responding to these items are respondents asked: "Do you approve or disapprove of the way Lyndon Johnson is handling his job as President?"

A respondent's approval or disapproval of the way Johnson was handling his job was likely to be biased by commitments to the preceding politically charged items involving Johnson. Without evidence to the contrary, it seems reasonable to assume that presidential approval items preceded by politically charged items are not comparable to presidential approval items not so preceded. A basic assumption of time-series analysis is that the dependent variable being followed longitudinally is presented consistently over time. Thus, there is a legitimate question whether polls taken before 1956 and during periods of the Johnson administration are comparable to other polls.

Sampling difficulties pose yet another difficulty for scholars who rely on the PPI as a dependent variable. Researchers consistently report the "significance" of various factors which might cause variation in poll results. Significance means the confidence with which one generalizes from a sample to a population. Researchers from Mueller on have gone to great pains to collect the *population* of relevant Gallup polls; there is no *sample* of polls. Instead, each poll is itself a sample from a population of adults. The statistical question is not whether one is accounting for "significant" amounts of variation in the population of polls; it is whether there is significant variation from poll to poll.

Consider the impact of events which might conceivably explain the variance among polls. Mueller hypothesizes that certain rally points increase a president's popularity (1973, p. 211). These are shown to explain a significant amount of variation in presidential popularity. Yet when the rally points not representing a change in administrations and for which there were polls available are examined for significant change (.05 level; see Glenn, 1970) associated with the rally event, 72 percent of the time there either is no such change (60 percent) or there is a significant *drop* in public approval (12 percent) for the president. Presidential popularity increases significantly on only 28 percent of these occasions. When testing for the impact of discrete events on presidential popularity, researchers at least should supplement their dummy variable regressions with a series of before-and-after examinations of the polls (see Hyman, 1972, p. 273).

Another difficulty for scholars relying on he PPI as a dependent variable is the fact that either polls nor events are discrete points in ime (as they commonly are treated). Polls and vents each have duration. By selecting a articular point in time to represent a poll or vent with duration, one risks confusing chron-logical sequence and, as a result, cause and ffect. Typically, it takes two days for Gallup olls to reach the field and interviewing to begin. Interviewing lasts another five days. Then it can take anywhere from a few days to months to compile and release the polls to the press. Representing such a process and/or the vent in question by a particular point in time, nd especially by publication date, is misleading.

Because Mueller's data are avialable, we will llustrate this difficulty with his treatment of he Cuban missile crisis and the Glassboro ummit. Respecting the missile crisis, about 40 percent of the interviewing for the 19 October "before" poll took place *after* the rally event, Kennedy's dramatic television speech. The "be-ore" poll nonetheless is coded to indicate a full year since a previous rally point. Respecting the ummit meeting, attributing Johnson's temporary high approval rate (52 percent) to the Glassboro meetings with Kosygin is equally problematic, as the event actually took place during the interviewing.

The Mideast Six-Day War seems a more ikely causal factor as it at least took place *before* interviews for the late June poll begin, while the Kosygin-Johnson meetings took place *fter* interviews begin. In both the Cuban missile crisis and the Glassboro summit meetng, temporal considerations make any attribuion of causality or tendency highly questionble. A more appropriate indication of the "rally" effect would be to compare responses within single polls: interviews before the rally event—Kennedy's speech, the Johnson and Koygin meetings—with interviews taken afterward. A chart showing the detail of rally events and poll dates is available from the authors.

Establishing the impact of events on presidential popularity is going to require careful, poll-by-poll analysis. Increasing the octane of he statistical techniques employed—as in Kernell's representation—while ignoring the weaknesses of the underlying data base will not get us very far.

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Reply

TO THE EDITOR:

The above statement by R. Darcy and Sarah Schramm challenges the appropriateness of multivariate regression analysis to study presidential popularity. Although my recent *Review* article provides the occasion for their letter, the thrust of Darcy's and Schramm's reservations apply more generally to this increasingly popular mode of inquiry. They propose, instead, that popularity research return to eyeballing the charts (i.e., "careful poll-by-poll analysis"). I find their misgivings overblown and their alternative research strategy simplistic.

The most serious complaint Darcy and Schramm raise concerns the potential non-comparability of the popularity ratings prior to 1956 and during certain periods of the Johnson presidency because of inconsistent placement of the presidential job performance question within the Gallup survey. In some instances this item followed other politically charged queries. Noting this, Darcy and Schramm casually argue that "without evidence to the contrary" we should "assume" that the popularity items in such cases are so "biased" as to be "not comparable." Unfortunately, no evidence is offered to support this assertion. One tell-tale sign that the ratings during the months in question are contaminated would be if the actual ratings systematically differ from the estimated ratings based upon the regression equations. An inspection of the residuals (from the equations in Table 5) for the potentially contaminated Johnson ratings failed to reveal any outliers (i.e., statistically significant misestimates) or a systematic pattern in either over or underestimating the approval level with the equation. Although some residual variance remains for these observations, as with virtually all of the ratings, there is little evidence of a serious contamination effect. But even if such a bias were present, this would provide little cause for abandoning econometric analysis. The effects of bad data here should be to reduce the overall relationships between presidential popularity and the exogeneous variables and to increase the variance in the error term. Given this likely result, Darcy's and Schramm's fears of contamination should lead to an even greater appreciation of the strength of the economy and war variables in explaining Johnson's decline in public support. (Darcy and Schramm also fail to acknowledge that they are impugning only part of the analysis since any measurement bias is isolated by separate equations for each administration.)

After several readings of their discussion of significance tests I remain unsure precisely what the authors are arguing. The concluding sentence of this paragraph appears to draw a false distinction between "poll-to-poll" variation and variation across a "population of polls." The latter seems to represent no more than a cumulative or general statement of the former.

Next, Darcy and Schramm complain strenuously about the "rally" variable. Here they appear to be directing their comments more to John Mueller's earlier *Review* (1970) article. They refer to his operational definition of "rally round the flag," his data series and rally points, and they ignore the relatively minor role of the rally variable in my analysis. Although I too had some reservations about Mueller's methodology, Darcy and Schramm are attacking what seems to me to be an important strength of his analysis. Resisting the temptation to first examine the bumps and grinds on the popularity charts before specifying the values of the rally events, Mueller wisely preferred to step back from the ratings and ask to what degree have dramatic, sharply focused, international events as a set determined the support levels of postwar, cold war presidents. Such a conservative approach has obvious costs. The estimates will represent an "average" effect with some events having a greater impact on approval than others. And, as I reported in "Explaining Presidential Popularity" (1978), the standardized, unweighted specification of disparate rally events appears in the residuals to account for some of the autocorrelation of the OLS estimates. A weighting scheme differentiating events that is validated on some independent criterion is attractive, but to base such weights upon the behavior of the dependent variable which Darcy and Schramm propose violates the integrity of the analysis.

This is not to argue that eyeballing before-after popularity ratings has no value. (I am, in fact, currently engaged in a study of the political and demographic sources of crisis support.) Poll-by-poll inspection of individual events' effects will, however, require a more sophisticated research strategy than Darcy and Schramm propose. They report from unpublished materials that most events are not followed by a surge of approval and some are actually attended by a decline in support. (See Lee, 1976, for more positive results.) But such a casual inspection ignores the fact that numerous forces are shaping public opinion at any given moment and the effects of an intervening rally event must be isolated from these other forces. One approach is to estimate the popularity equations with the rally term omitted (its correlation with the other exogeneous variables is low) and then inspect (or better still, perform an interrupted time-series analysis upon) the residuals.

I share Darcy's and Schramm's concern with the prior issue of the validity and reliability of one's variables before performing "high octane" statistical analysis. I think, however, that time-series analysis of postwar support for the president is on safer ground than the authors argue.

I wish to thank Harvey Zeidenstein for catching a typo in Table 5. The regression coefficient for N.V. Bombing should read .0003 which conforms to the estimate in the text and Tables A–1 and A–2.

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Comment on LeLoup (Vol. 72, June 1978, pp. 616–21)

TO THE EDITOR:

In a June 1978 *Review* article, "Reassessing the Mediating Impact of Legislative Capability," Lance LeLoup offers an interesting conceptual alternative to Carmines' (1974) analysis of the influence of legislative professionalism on the policy impact of state party systems. Both studies reflect a healthy concern for the impact of political institutions on policy outcomes. However, the empirical evidence which LeLoup presents to verify his hypothesis is subject to two very serious shortcomings which cast doubts on the validity of the results. Those shortcomings involve an inadequate control variable and unequal variances in the competition measure when the states are grouped according to legislative capability.

The first difficulty arises from the control variable used by LeLoup. The variable he used is a rank order (see LeLoup, 1968, p. 618, n. 6). Regression analysis assumes interval-level measurement. Thus, it is not surprising to find less socioeconomic variance accounted for by the socioeconomic index used by LeLoup than when we use the more precise interval-level measures, such as percent urban, median income, and the percentage of the labor force employed in manufacturing. And, since competition correlates with socioeconomic traits, we should expect competition to have greater impact in LeLoup's study than is generally found in other studies in the state policy outcomes field. Consequently, as we will show, our expectation is borne out.

The second difficulty arises when the states are grouped according to their rankings on the legislative capability measure. When that is done, Florida is the only state of the Deep South to fall into the high legislative capability group. Further, all of the states which Ranney (1971) classifies as one-party states (Louisiana, Alabama, Mississippi, South Carolina, Texas, Georgia, and Arkansas) fall into the low-capability group. As a result, the standard deviation of the folded Ranney measure of competition used by LeLoup is .078 for the high legislative capability group but is .158 for the low legislative capability group. That competition should have more policy impact in the latter group is therefore not surprising. In effect, LeLoup compares two groups which he asserts are systematically different only in terms of legislative capability,[1] but which also systematically differ in terms of interparty competition.

To summarize, the combination of uncontrolled socioeconomic variance and the unequal variance in the competition measure when the states are grouped according to legislative capability means the apparent influence of competition between high and low legislative capability states is also overstated and the difference in the influence of competition between high and low legislative capability states is also overstated.

Analysis based on 1970 policy measures confirms that the patterns found by LeLoup are due in large measure to these two problems. First, results of comparisons of the beta coefficients between competition and various policies in the high and low legislative capability states tend to match those found by LeLoup when his socioeconomic control variable is used.[2] However, when urbanization, median income, and the percentage of the nonagricultural work force employed in manufacturing are used as controls, in place of the control used by LeLoup, which was based on similar components, support for his hypothesis weakens (see Table 1). Only the two education measures provide any support, and the tendency for the welfare measures is in the opposite direction. The results indicate that at least some of LeLoup's findings are due to the inadequacy of the control variable used in his analysis.

What support remains for LeLoup's hypothesis is virtually eliminated when the southern states are omitted in order to equalize the variance of the competition measure in the two capability groups. The welfare measures contradict the hypothesis that competition has more impact in states with low legislative capability, as does expenditures per capita for highways. Innovation and tax progressivity reveal no legislative capability influence, and education expenditures per pupil suggests negligible influence. Only education expenditures per capita provides any support for the hypothesis, but the support is modest.

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[1] LeLoup (1978, p. 618) checked the unstandardized coefficients as a safeguard against variance problems, but the check apparently failed to detect the problem.

[2] Inspection of the first two columns of Table 1 shows that even when using LeLoup's variables, we are unable to replicate his results consistently. One difference between his data and ours is our use of 1970 data for the policy measures, while he used 1971 data. Such a difference should only produce minor inconsistencies due to the high stability of state policy patterns over the short run and incremental decision making in budgetary politics.

Recent studies which have sought to incorporate political institutions into the analysis of state policy outcomes are praiseworthy because they may yield a more comprehensive understanding of the factors affecting policy outcomes and the manner in which various influences are exerted. However, analysts must proceed with great care in order to assure that the results are not reflections of variance problems or inadequate control variables.

The lack of support for LeLoup's hypothesis may reflect several things. First, legislators in the less capable legislatures may indeed be more dependent on the party as an information source than are legislators in the more capable institutions, but the latter may have more control over their political environment. The net effect may be that differences in dependency are offset by differences in control. The legislator in a less capable legislature may have the incentive to enact progressive programs but may lack the institutional means with which to develop them. Therefore, high legislative capability may exert a mixed impact on party influence because it sometimes becomes a party resource and in other circumstances becomes a personal resource. Formal characteristics may conceal a variety of informal adaptations.

DAVID NICE
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Table 1. The Mediating Impact of Legislative Capability: The Impact of Interparty Competition on Selected Policies

	Interparty Competition[a]					
	Legislative Capability					
	High[b]	Low[b]	High[c]	Low[c]	High[d]	Low[d]
Tax Progressivity	.10	.18	–.01	–.08	–.14	–.03
Old Age Assistance Payments	.34 (.09)	.12 (.18)	.09	–.34	.07	–.40
Aid to Dependent Children Payments	.49 (.29)	.53 (.58)	.24	.03	.07	–.34
Education Expenditures per Pupil	.10 (.15)	.61 (.53)	–.16	.10	–.11	.12
Education Expenditures per Capita	.41 (.12)	.68 (.31)	.18	.38	.11	.33
Welfare Expenditures per Capita	–.10 (.03)	–.07 (.18)	–.02	–.22	–.19	–.49
Highway Expenditures per Capita	.42 (.12)	.51 (.64)	.18	.08	.22	–.12
Innovation (Walker)	.23 (.00)	.21 (.22)	.07	.07	.00	.02
N =	24	24	25	25	20	20

Sources: Austin Ranney (1971), "Parties in State Politics" and Jack Walker (1971), "Innovation in State Politics," in Herbert Jacob and Kenneth Vines (eds.), *Politics in the American States,* Boston: Little, Brown, p. 87; Citizens Conference on State Legislatures (1971), *Report of an Evaluation of the 50 State Legislatures,* Kansas City, Mo., pp. 29, 82–83; Duane Lockard (1968), "State Party Systems and Policy Outputs," in Oliver Garceau (ed.), *Political Research and Political Theory,* Cambridge, Mass.: Harvard University Press, pp. 217–18; Council of State Governments (1972–73), *Book of the States,* Chicago, pp. 211, 311, 381, 383.

[a] Competition is measured by the folded Ranney (1971) index, which was used by LeLoup. The coefficients are betas from multiple regression.

[b] The composite economic index used by LeLoup is controlled for. Items in parentheses are LeLoup's findings, and the others come from our replication using 1970 data for the policy measures.

[c] Percent urban, median income, and the percentage of the nonagricultural work force employed in manufacturing are controlled.

[d] The 10 states of the Deep South are removed. Percent urban, median income, and the percentage of the nonagricultural work force employed in manufacturing are controlled. The cutting point between high and low legislative capability was moved to produce groups of equal size.

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Comment on Butler
(Vol. 72, March 1978, pp. 135–50)

TO THE EDITOR:

Melissa Butler's attempt to find the "early liberal roots of feminism" in John Locke's "attack on patriarchy" is, although interesting, ultimately unsuccessful. This is so for at least two reasons, the first having to do with faulty logic, the second with skewed evidence. I will briefly explicate the first and illustrate the second.

The claim that Locke was a proto-feminist rests implicitly upon the following argument: Locke was opposed to patriarchy; modern feminists are opposed to patriarchy; ergo, Locke was a (modern, pre-modern or proto-) feminist. This is of course the shakiest of syllogisms; it takes no great logical acuity to recognize the argument as a non sequitur. Employing the mode of argument often used to establish guilt-by-association-of-ideas (e.g.: communists oppose fascism; F.D.R. opposed fascism; ergo, F.D.R. was a communist), Butler wishes instead to establish credit-by-association. Unfortunately, to credit Locke for his alleged feminist views is to give credit where none is due.

Butler rather misses the point of Locke's "attack on patriarchy" in the *Two Treatises*. Locke wished to show that *familial* relations provide us with no workable model of *political* association. In this respect his arguments parallel Aristotle's. Indeed, the comparison is instructive inasmuch as Aristotle—certainly no feminist—had also criticized the patriarchal model. In the *Politics* Aristotle argues (*contra* Plato) that the rule of a husband/father over his wife and children is unlike the statesman's rule, inasmuch as the former's superior strength and virtue renders him naturally fit to rule his inferiors: "The male is by nature superior, the female inferior; the one rules, the other is ruled" (*Politics*, 1254 b; cf. also 1259 a–b, 1260 a, and 1277 b). Because political relations are relations among citizens, i.e., equals, the patriarchal-familial model does not apply. To think of the *polis* as an extended family or household is to misconceive the structure and purpose of both. The patriarchal rule that is "natural" in the household is "unnatural" or "perverted" when transposed to the *polis*.

Locke makes an argument not far removed from Aristotle's. Locke, too, wished to demonstrate the political irrelevance and inapplicability of the patriarchal model—a model that finds its proper and "natural" sphere in the family, that preeminently private association ruled by a dominant male: "But the Husband and Wife, though they have but one common concern, yet having different understandings, will unavoidably sometimes have different wills too; it therefore being necessary that the last Determination, *i.e.* the Rule, should be placed somewhere, it *naturally* falls to the Man's share, as the abler and stronger" (*Second Treatise*, § 82, emphasis added). And elsewhere he says that "there is . . . a Foundation in Nature" for "the Womans Lot," namely "that she should be subject to her husband" (*First Treatise*, § 47). Such remarks Butler either brushes aside or quotes with a commentary to show that Locke does not actually mean what he appears to mean and so is not the benighted male supremacist that he appears to be (see Butler, 1978, esp. p. 145). But alas, these and other of Locke's remarks, far from representing the momentary lapses of an otherwise faithful feminist, point to a markedly anti-feminist "Aristotelian" strain in his thought. In the end, Locke is no more of a feminist than is Aristotle, and for precisely the same reason. Locke's view of the "naturalness" and propriety of patriarchy *in the family* is scarcely more than a paraphrase of several passages in Aristotle's *Politics*. Both agree that the patriarchal shoe fits the familial foot and should accordingly be worn at home —and nowhere else.

In the *Two Treatises* Locke is utterly uninterested in conjugal and familial relations per se. But when he does turn his attention to marriage, he is—*pace* Butler—anything but a progressive proto-feminist. An illustration may suffice. Butler (1978, p. 144) avers that Locke's having "suggested the possibilities of left-hand marriage" supports her claim that Locke is a proto-feminist. But one need only examine the context and content of Locke's utterance to see how mistaken she is. As to context: Locke's remark has nothing whatever to do with his supposedly pro-feminist "attack on patriarchy"; indeed, it occurs in a diary entry dated

1678–two years *before* the publication of Filmer's *Patriarchia* and well before Locke's writing of the *Two Treatises* (on the difficulties of dating their composition precisely, see Laslett's Introduction to the *Two Treatises* [1960], esp. p. 59). In addition to wrenching Locke's remark from its historical context, Butler paraphrases it so as to support her "feminist" reading of Locke; unfortunately this reading is a misreading. What Locke *actually* said–and presumably meant–was this: "*He* that is already married may marry another woman with *his* left hand," adding: "The ties, duration, and conditions of the left hand marriage shall be no other than what is expressed in the contract of marriage between the parties" (*Diary*, 1678, quoted by Laslett [1960], p. 339, n., emphasis added). Locke says nothing about extending this prerogative to women; on the contrary, he suggests that left-hand marriages may take place only between an already-married man and a single woman, with the first wife having little or no say in the matter. In this respect, as in so many others, Locke is assuredly no feminist; he is, on the contrary, a dyed-in-the-pigskin male chauvinist.

In spite of a game and interesting attempt, Butler fails to support in any convincing way her claim that modern feminism has "early liberal"–specifically, Lockean–"roots." She finds these roots only, I submit, by uprooting Locke and by treating him with undeserved generosity. Better, I think, to let sleeping chauvinists lie. So far as modern feminism is concerned, the soil in which Lockean liberalism took root is acidic and barren. Those of us interested in unearthing the roots of contemporary feminism might be better advised to dig elsewhere.

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Reply

Instead of reducing the argument of "Early Liberal Roots of Feminism" to its "implicit syllogism," Terence Ball has actually substituted and subsequently attacked an argument of his own making. His effort shows that he missed the point of the article. Consequently, his criticisms, too, miss their mark.

It certainly was not the contention of my article that Locke and feminism could be linked simply because both opposed patriarchy. Rather, any connection between the two resulted from the particular way in which Locke chose to attack patriarchy–namely by emphasizing individualism. As Ball suggests, Locke, like Aristotle, distinguished between political and household relationships. In doing so, Locke did at times borrow freely from Aristotle. Yet these borrowings were not "the momentary lapses of an otherwise faithful feminist" (Ball's interpretation, which, incidentally, goes far beyond any claim made in the article); rather they may be seen as the lapses of a reasonably faithful individualist who generally took his individualism seriously even where women were concerned.

Locke followed Aristotle in rejecting paternal power as the model for political power, but he certainly departed from Aristotelian organicism by viewing political society as a limited-purpose association based on the consent of individual members. Furthermore, Locke recast other adult human relationships in similar terms. The hallmark of most relationships among adult human beings was individual consent. Though other relationships would differ from the political relationship in their ends and in the nature of the power conferred, they would share a contractual base. Indeed, the only way to bind adult individuals was through their own consent. The single exception to this rule was the master-slave relationship, yet on that score, Locke clearly distinguished himself from Aristotle. A person became a slave when he had "by his fault, forfeited his own Life, by some Act that deserves Death" (*Second Treatise*, §23). The slave's situation was not the result of a natural difference in rationality, nor was it rooted in contract, yet it was clearly the product of a slave's own individual action.

The relationships of husband and wife, master and servant were clearly grounded in voluntary individual actions of contract and exchange. The discussion of the contract between husband and wife is found in several passages following the "Aristotelian" section Ball discussed. In these passages, Locke allowed for a great deal of freedom and variability in the terms of the marital contract. He included the possibility of limiting the duration of marriage and also of allowing women to separate from their husbands while retaining custody of the children. The limits of "nature" in the conjugal relationship were blurred considerably when Locke asserted that women could hold property and have interests apart from their husbands' and over which their husbands acquired no rights. Women's individuality could not be completely submerged through marriage. This position was a far cry from the one expressed earlier in the same century in *The Lawes Resolution of Women's Rights* where it was explained that "in this consolidation which we call wedlock is a locking together. It is true, that a man and wife are one person; but understand in what manner. When a small brooke or little river incorporateth with Rhodanus, Humble, or Thames, the poor little rivulet looseth her name; it is carried with the new associate; it beareth no sway; it possesseth nothing during coverture."

The notions of individual choice, contract and variability were also at the heart of the diary entry Ball found so troubling. What was important there was that the association was clearly to be based on conditions to be fixed by the parties themselves. The discussion of left-hand marriage in the *Atlantis* diary entries was introduced as evidence that Locke had given some thought to alternatives to traditional relationships. That he may have done so before reading Filmer does not weaken that point in the least, rather it serves to indicate the kind of mindset he brought to his reading of Filmer. Furthermore, it should be noted that even prior to his reading of *Patriarcha,* Locke was certainly acquainted with the basic argument of patriarchal theory (see the 1661 manuscript of *Two Tracts on Government* [Locke, 1967, p. 230]).

In short, the connection between Lockean liberalism and the development of feminism is rooted in Lockean individualism—an individualism which even Locke himself applied to women. If Lockean theory can still be said to underlie the American Declaration of Independence of 1776, the 1848 Seneca Falls Declaration of Sentiments was no less rooted in Lockean soil.

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Reference: Locke, John (1967). *Two Tracts on Government.* Edited by Philip Abrams. Cambridge: Cambridge University Press.

Comment on Brams and Fishburn (Vol. 72, September 1978, pp. 831–47) and Balinski and Young (Vol. 72, September, 1978, pp. 848–58)

TO THE EDITOR:

It is highly desirable that we canvass new and different methods of voting and carefully consider the mathematical consequences of the existing methods. The Brams and Fishburn, Balinski and Young articles in the September 1978 *Review* are, then steps in the right direction. Each, however, has a defect.

Beginning with Brams' and Fishburn's (1978) approval voting: most economic societies elect some of their officers by a method which resembles approval voting. There are, for instance, two vice-presidents of the Southern Economic Association elected each year. The nominating committee nominates four persons and the individual voter has two votes. If the voter chooses to vote for only one, however, that vote is counted. This process leads to very considerable strategic maneuvering since obviously if you vote for two you reduce the likelihood of your first preference candidate winning. Thus, the decision whether to vote for two candidates or cast a "bullet" vote is a matter of careful consideration to those people who give careful consideration to any aspect of the balloting.

The approval voting scheme would be subject to the same kind of strategic considerations. Only one candidate would be elected but the strategic considerations would be the same. Brams and Fishburn, however, define "strategic" in such a way as to exclude this particular type of strategy. Indeed, voting for either one or two candidates would be classed as "sincere."

It is fairly easy to work out examples in which the outcome would be very very seriously affected by voter strategies similar to those used by many voters in the election of vice-presidency in the Southern Economic Association. The existence of this kind of strategic choice for the voter should be weighed in considering approval voting not defined out of existence.

Turning to the Balinski and Young article on proportional representation (1978): the problem they deal with is rounding error. All of their paradoxes come from different ways of dealing with the rounding problem. They say, "Exact proportionality cannot, in general, be achieved since representation must be integral" (p. 848). No doubt, in the mid-nineteenth century representation did indeed have to be integral, but we now have all sorts of electronic devices and there is no reason why the individual parties cannot be given exactly the same number of votes in the legislature as they received in the general election (see Tullock, 1967, pp. 144–46). The fact that it is possible does not, of course, prove that it is desirable, but the issue should be discussed rather than swept under the rug.

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State University

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Brams, Steven J., and Peter C. Fishburn (1978). "Approval Voting." *American Political Science Review* 72: 831–47.

Tullock, Gordon (1967). *Toward the Mathematics of Politics.* Ann Arbor: University of Michigan Press.

Replies

TO THE EDITOR:

Gordon Tullock is right that we classify "bullet" voting as sincere, but only if the bullet is cast for a voter's most-preferred candidate. Under plurality voting, on the other hand, if there are, say, three candidates, it may be rational to vote for a second choice. This is never the case under approval voting—one would vote for only one's first choice, or first two choices. That is why we consider approval voting a more sincere voting system. Given that virtually no system is invulnerable to strategic calculations, we believe the quantitative question of which system does best according to our criteria of sincerity and strategyproofness to be an important one.

STEVEN J. BRAMS

New York University

PETER C. FISHBURN

Bell Laboratories

TO THE EDITOR:

The letter to the editor by Gordon Tullock suggests that while mid-nineteenth century proportional representation advocates insisted on integral representation, today's electronic mid-twentieth century legatees might well prefer rational representation which is exactly proportional to the vote totals of each party. It accuses our article (Balinski and Young, 1978) of harboring the defect of having swept discussion of this possibility "under the rug."

Our article deals *only* with the phenomena which arise when integral representation is demanded. If this constraint is abandoned, then our article should be as well. The overwhelming majority of proportional representation (and plurality) systems used today give to each elected representative one seat and one vote. The decision of how many seats to mete "proportionally" to party vote totals is made via differing rules. It is therefore of interest to gain a fundamental understanding into how these rules function.

The aim of our article is not to play with "paradoxes," but rather to explore the underlying properties of methods in order to provide a theory of allocation through which methods can be analyzed, compared and hence ultimately chosen.

One can, of course, study models in which a party vote total is somehow translated into a proportional number of votes in a legislature, or a shareholder casts a number of votes equal to the number of shares owned. The difficulty with such schemes is that their implications for actual voting strength are far from obvious. But such studies have, for example, led to a 1971 reform of the Nassau County Board of Supervisors (Long Island, N.Y.). This Board represents six municipalities of rather vastly differing populations, and it was noticed that a roughly proportional representation implied that the two relatively small municipalities, each having two votes, were actually "powerless" since their votes could never be essential to any majority decision (a majority being sustained with or without these votes in all possible cases). The reform changed the decision mechanism by insisting on more than a simple majority of votes for a motion to pass.

Different schemes can be envisaged, including giving each party (or municipality) a number of votes directly proportional to its numbers: but many schemes are possible. Which are feasible? Should all votes be given to the party leader? Or be divided, in some equal or unequal manner, between a small number of party elders? Tullock makes no specific suggestion. Each option carries mathematical implications as well as psychological ones. The role of mathematics—as in our article—is to investigate the underlying consequences of adopting one or another practical and acceptable procedure.

In conclusion, such problems are fascinating and deserve attention—and their study should contribute to an understanding of the desirability of some form of exact proportionality in PR systems—but they are simply not the problems which were the object of our work, and hence were not discussed. The object of our attention was the fair division of seats among parties, each seat having the right to one vote, a not uncommon situation.

M. L. BALINSKI
H. P. YOUNG

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Reference: M. L. Balinski and H. P. Young (1978). "Stability, Coalitions and Schisms in Proportional Representation Systems." *American Political Science Review* 72: 848–58.

EDITORIAL NOTE

The following articles have tentatively been scheduled to appear in the September, 1979, issue:

Charles W. Anderson, University of Wisconsin, "The Place of Principles in Policy Analysis"

Helmut Norpoth, University of Arizona, "The Parties Come to Order! Dimensions of Preferential Choice in the West German Electorate, 1961–1976"

Paul Allen Beck, University of Pittsburgh, and M. Kent Jennings, University of Michigan, "Political Periods and Political Participation"

Teresa E. Levitin and Warren E. Miller, University of Michigan, "Ideological Interpretations of Presidential Elections"

Diane L. Fowlkes and Jerry Perkins, Georgia State University, and Sue Tolleson Rinehart, Rutgers University, "Gender Roles and Party Roles"

John L. Sullivan, University of Minnesota, James Piereson, University of Pennsylvania, and George E. Marcus, Williams College, "An Alternative Conceptualization of Political Tolerance: Illusory Increases 1950s–1970s"

Ronald Randall, University of Toledo, "Presidential Power versus Bureaucratic Intransigence: The Influence of the Nixon Administration on Welfare Policy"

Richard Born, Vassar College, "Generational Replacement and the Growth of Incumbent Reelection Margins in the U.S. House"

Erratum

The review of *Policy Studies and the Social Sciences*, edited by Stuart S. Nagel, in the December 1978 *Review* (pp. 1381–83) incorrectly listed Lexington Books as the publisher of both the cloth and paperback editions. Lexington is the publisher of the cloth edition of the book, but the publisher of the paper edition is Transaction Books.

BOOK REVIEWS

Political Theory and Methodology

Forecasting: An Appraisal for Policy-Makers and Planners. By William Ascher. (Baltimore: Johns Hopkins University Press, 1978. Pp. xiii + 239. $15.00, paper.)

Although the title of this book suggests that its contents are for policy makers and planners, the volume also has relevance for a broader readership, including statements of public administration, organizational and bureaucratic behavior, and American politics generally. The core question of the book is forecast accuracy. In appraising accuracy, Ascher examines forecasts in five areas: population, the economy (current dollar and real GNP), energy (electricity consumption, petroleum consumption, total energy consumption, petroleum reserves), transportation (general aviation, commercial airline travel, motor vehicles), and technology (nuclear energy, computer capabilities, innovations in automation, scientific breakthroughs). Ascher concludes, after examining forecasts in all five areas, that the most significant factor in determining accuracy is the time horizon of the forecast; the more distant the forecast target date, the less accurate the forecast. Ascher also finds that the institutional sites of forecasts tend to be associated with systematic biases. Institutional affiliation and sponsorship tend to influence the core assumptions used in generating forecasts and consequently introduce systematic biases. Ascher also isolates a temporal bias in forecasting: forecasts made at about the same time tend to share assumptions (either correct or incorrect) and consequently tend to produce similar projections.

These findings should surprise no one. Indeed, the bulk of the volume substantiates what many have accepted as true about expert forecasts, albeit without systematic and carefully appraised evidence. However, two of Ascher's findings counter what many forecasters have accepted as conventional wisdom. Ascher finds that the choice of forecasting methodology makes little or no difference in accuracy. Ascher points to two factors which seem to further substantiate this empirical finding. First, forecasters are prone to use multiple methods, in some combination, seemingly for the purposes of compensating for weaknesses in any single methodology. Second, the presumed advantages of sophisticated methodologies, especially those based on mathematical modeling techniques, have not materialized. For reasons that Ascher only partially explores, it seems that both the producers and consumers of forecasts have been reluctant to invest resources in forecasts based on the more recent mathematical methods in systems science, such as linear and dynamic programming, optimal control and catastrophe theory. These and other refined methodologies are not in wide use and have been relegated to the pages of academic journals.

A second of Ascher's findings which may surprise some forecasters is that the accuracy of forecasts, in part, seems to rest on some presumptions forecasters make about their data. In generating forecasts, commonly input data includes the results from some other forecasts. This interrelationship between forecasts—for example, energy consumption forecasts often assume as accurate the output of population forecasts—accounts, in part, for errors. In addition, core assumptions are often assumed to be true long after there is ample evidence to demonstrate their falsity. Forecasters often rely on antiquated information about the underlying dynamics of the phenomena about which they are forecasting. In other words, many of the advances made in the basic sciences are not contained within the forecaster's assumptions. Scientific communication between forecasters and basic researchers is inadequate. As Ascher notes, when core assumptions fail to correspond satisfactorily with empirical realities, methodology cannot save the forecast. Although it is plausible that wrong assumptions may yield true forecasts—and believe it or not, Ascher has found some delightful examples of this phenomenon—correct core assumptions are crucial for forecasting accuracy.

Although the bulk of Ascher's book deals with forecasting accuracy and in these chapters he reasons with care, precision and rigor, Ascher does offer a few observations on the forecasting business. It is here where the book suffers its greatest problems. The beginning and concluding sections of the volume attempt to conceptualize forecasting as a relationship between producers and purchasers (audiences). As Ascher notes, the "best" forecast is not necessarily the most accurate one. Questions of use, interpretability, timing, audiences, normative judgments, and other issues, all seem to make forecasting accuracy less salient as an ultimate goal. Whether forecasters are employed by governments, industry, or by other sectors of society, many factors, some probably beyond the cognitive awareness of forecasters, seem to be part and parcel of the business. In essence, Ascher sees no truly objective forecast, but only the existence of forecasts relative to the relationships which generate them. In fact, Ascher admits that accuracy may be subservient to other objectives. To understand the forecasting business, therefore, questions beyond simple accuracy need to be asked; analyses of social psychology and economics seem to be called for. Although Ascher offers some observations, more as historical asides, on forecaster-client relationships, these key elements in his conception go unanalyzed and unprobed. Accuracy, as Ascher argues, is the most readily available criterion for judgment. However, accuracy, as Ascher admits, is not necessarily the most salient criterion for gaining a handle on the forecasting enterprise. A justification of accuracy as a suitable criterion for evaluation in and of itself would better motivate the study. A defensible justification of accuracy as a sole criterion may well not exist.

Although one might quibble with the forecasts Ascher has chosen to examine, especially the exclusion of the massive enterprise of forecasting in defense-related matters, one cannot quibble with the overall quality of the work. Although only offering very partial understanding of the forecasting business, Ascher tackles the problem of accuracy with solid presentations. His methods of assessment and presentation of the methods of a host of forecasting studies are impeccable. Indeed, for forecasters and forecast-watchers alike, Ascher's book is mandatory reading. For skeptics, the book will give sufficient reason to continue past skepticism about forecasting. For forecasting pundits, the book provides arguments for discussion and refutation. For forecasters themselves, the book amply shows the vacuous and often unsophisticated nature of the enterprise. The book sympathetically challenges forecasters to reach for a higher standard of excellence. For the consumer of forecasts, the book provides grounds for letting the buyer beware.

JOHN V. GILLESPIE

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Workplace Democratization: Its Internal Dynamics. By Paul Bernstein. (Kent, Ohio: Kent State University Press, 1977. Pp. 127. $12.50.)

This work is a monograph prepared by the Comparative Administration Research Institute of Kent State University. Paul Bernstein is the new co-director, with Severyn Bruyn, of the new graduate program in workplace democratization of Boston College. The present volume is the first book-length exposition of Bernstein's conception of this increasingly important area of organizational theory, representing a more comprehensive statement than the several valuable articles Bernstein has published on workplace democratization in the last few years.

An initial section discusses such familiar cases as Northwest Plywood Cooperatives, Scanlon Companies, the American Cast Iron Pipe Company, and Scott Bader Commonwealth. This section provides no new information to interest specialists, but is a convenient summary for organization theorists who may be unfamiliar with the literature on workplace democratization.

The major section of the monograph concerns modeling the internal dynamics of workplace democratization. Here Bernstein has arranged by chapter his treatment of what he takes as the six major prerequisites of successful organizational democracy: participation and decision making, economic return, sharing management-level information, guaranteeing individual rights, establishment of an independent judiciary, and inculcation of a consciousness of participatory democracy. A final section presents a brief overview and summary of the model.

Bernstein's conception of organizational participation will no doubt be much criticized by organization theorists for its failure to integrate relevant portions of what is now often called contingency theory. Likewise, the literature on organization development/design is substantially ignored. On the other hand, Bernstein's typological conception of the issues of organizational democracy deserves the attention and appreciation of organization theorists. His rank-order measurement of such variables as influence degree, issue range, organizational level, and other variables is, in my judgment, more directly applicable to operational research on labor participation than concepts currently prevalent among organization development specialists and other policy analysts ostensibly interested in issues of organizational democracy. From a political science viewpoint, Bernstein is particularly helpful in directing attention to questions of power which have often been obfuscated in organization theory literature originating in other disciplines.

Bernstein's sections on the guarantee of individual rights and quasi-judicial functions within the enterprise represent an important contribution to the discussion of workplace democratization. These issues, all too often neglected, are indeed critical prerequisites to successful workplace democratization. Again, however, the brevity of the monograph sections will leave many dissatisfied with the lack of comparison with existing quasi-judicial safeguards in collective bargaining and administrative law.

In this work Bernstein has focused on autonomous profit-making enterprises which are devoid of a communitarian component. In so doing he has ignored such important cases of workplace democratization as those centered on nationalized enterprises, whether of the Yugoslav sort or the Chinese, the kibbutz industry examples, and the legion of communitarian and utopian experiments. By intention then, this monograph on "workplace democratization" presents a model useful in describing only a certain range of experiments. In particular, state-initiated and union-initiated democratization attempts are not treated. Since, in my opinion, these represent the most important attempts, Bernstein's model of workplace democratization is rather limited. Worker-labor and labor-state relations pose the most perplexing and interesting questions in organizational democracy. By choosing to downplay state-initiated and labor-initiated experiments Bernstein seems to implicitly hold up as an ideal the model of spontaneous, worker-initiated experiments. This implies an almost utopian model of social change which Bernstein does not intend, yet to which he presents no alternative.

In summary, this short book provides a valuable introduction to organizational democracy, particularly as regards the consideration of preconditions and typological distinctions. For this purpose it far surpasses the nearest alternative that comes to mind, Gerald I. Susman's *Autonomy at Work.* On the other hand, Susman's familiarity with literature on organizational theory and development gives that work a depth not present in Bernstein. The contrast suggests the need in future research to arrive at a synthesis between these two important and challenging orientations: sociotechnical analysis (Susman) and democratic theory of work (Bernstein).

G. David Garson

North Carolina State University

Game Theory and Politics. By Steven J. Brams. (New York: Free Press, 1975. Pp. xix + 312. Price not given.)

Paradoxes in Politics: An Introduction to the Nonobvious in Political Science. By Steven J. Brams. (New York: Free Press, 1976. Pp. xvii + 231. Price not given.)

These volumes are specimens of a species that has an established niche in natural science, although it is rare in political science. They are advanced textbooks. The basic research has been reported previously, in scholarly journals, by diverse authors; and, aside from organization and style, the content is original only when that is required for a consolidated account.

The journal literature surveyed and summarized in these volumes is decision theoretic or pure theory; the empirical world is rarely its starting point. Postgraduate students should be delighted by these summary expositions. Specialists are likely to find references to results that they have overlooked. Furthermore, although this genre can scarcely be read intelligibly by introductory students, instructors should mine the volumes like intellectual quarries for lectures on the electoral college, congressional apportionment, presidential electioneering, and other topics in the elementary course on American Government.

Paradoxes in Politics discusses sophisticated voting, cyclical majorities, prisoners' dilemma, sequential primaries, congressional apportionment, voting power, prediction problems, and other topics. *Game Theory and Politics* considers international relations games and diverse voting games. *Paradoxes* provides more varied fare; about one-third of its content is drawn from *Game Theory.*

Both volumes are distinguished by judicious comments on the relevant literature. Brams, in passing, corrects various errors that have crept into print. Even Homer nods, however, and stray flaws have also crept into these books.

Arrow's General Possibility Theorem is occasionally misconstrued in both volumes. This blemish is forgivable since there is a virtual tradition of such misstatements, but the point deserves amplification.

Kenneth Arrow himself construed his theorem too casually, in prose, when he remarked that "there is no method of voting which will remove [the possibility of] the paradox of voting, . . . no matter how complicated" (*Social Choice and Individual Values*, Vol. 3, words in brackets added). Arrow's original passage, sans my interpolation, has been carelessly echoed by many writers. Brams, for example, writes of "the ironclad guarantee . . . that the paradox of voting will occur" (*Paradoxes*, p. 54). Arrow's five conditions are said to "ensure the occurrence of the voting paradox" (*Paradoxes*, p. 35; *Game Theory*, p. 126). This language, although commonplace, is misleading at best and erroneous at worst.

The General Possibility Theorem does *not* imply that the paradox must occur, the paradox necessarily occurs, or the paradox always occurs. (Consider three alternatives and three voters with identical preference schedules.) The theorem does imply that the paradox must be possible in a democracy, the paradox may occur in a democracy, and the paradox cannot be proscribed in a democracy. (Consider three alternatives and three voters with permuted preference schedules.)

Brams correctly states its essence: "All methods of summing individual preferences that satisfy the five conditions . . . for *some* preference scales of individuals . . . will necessarily lead to cyclical majorities" (*Paradoxes*, p. 34; emphasis added). His conclusion that "the paradox of voting cannot be avoided," thus simply confounds the existential and universal quantifiers. This flaw, fortunately, does not vitiate the commentary by Brams.

These books, in sum, are very useful. All academic libraries should have copies. Every postgraduate student should have copies. The contents should be pondered by all political scientists.

THOMAS W. CASSTEVENS

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The New Liberalism: An Ideology of Social Reform. By Michael Freeden. (Oxford, England and New York: Clarendon Press, 1978. Pp. xii + 291. $24.00.)

Michael Freeden's *The New Liberalism* is both a good study of the philosophy and politics of the new liberalism and a good example of new liberal thinking. Consequently, it is an excellent source for investigating the strengths and weaknesses of those who would defend a more rational welfare state.

The ostensible focus of the study is a group of intellectuals writing and participating in English politics at the turn of the twentieth century. Well-known thinkers like Green and Hobhouse are grouped with lesser-known luminaries like Samuel and Masterman; and Hobson is portrayed as the star of the piece. What unites these people is their concern to reconcile the old contractarian, individualist, laissez-faire liberal traditions with the newer utilitarian, idealist and biological theories based on individual sociality, a positive state and a new liberal man. The trick, of course, is to be able to do it without being labeled a socialist (though a number of Fabians do slip through Freeden's new liberal web).

At the level of describing, analyzing and documenting their thought, Freeden is quite masterful. The influences of the later Mill, Darwin and others provide a rich stage upon which Freeden introduces his players and demonstrates the cohesiveness, comprehensiveness and influence of the new liberalism. These are not piecemeal reformers; their systematic philosophy and progressive political platform are "organically" related as are individuals and the state. Moreover, these are not piecemeal borrowers; their ideas are developed independent of others despite the similarities. These are the people who transformed liberalism from an early capitalist ideology to one capable of confronting and solving "the social problem" spawned by capitalism. Freeden has indeed convinced me that these unsung heroes are the intellectual sires of the modern welfare state.

But why, I wondered while reading the book, is he so intent on resurrecting these heroes, on distinguishing them from other liberals and socialists, or on implying that the demise of the liberal party came because they refused to listen to the new liberals? The answer was readily available; Freeden is a new liberal himself. His emphasis on the narrow working-class base of the Labour party and the old liberalism of the Conservatives, plus his constant references to problems which the new liberals confronted yesterday which remain unanswered today, indicate his belief that the new liberalism provides answers for today's political dilemmas. In the United States, he might be called a new populist.

It seems that the problem with the welfare state is that its new liberal sires were never able to nurture its development. Instead socialists and old liberals created a mutant which has not replaced class conflict with class harmony, bureaucratism with rational administration, or atomistic individualism with sociopolitical organicism. Though Freeden nowhere states this explicitly, it is an intelligible conclusion to draw from his study. And it is not convincing.

That his new liberals struggled with many of the problems we face today is undeniable. Their early recognition of these problems (e.g., unemployment, old-age pensions, health care, etc.) is laudable. Nevertheless the usefulness of their ideas and programs is questionable. They do not help us understand how individuals can be organically related to the state when both are dominated by national and international corporate interests. Nor do they help us solve "the social problem." State regulation, arbitration and even ownership within the context of advanced capitalist societies have failed to eliminate unemployment, poverty or crime. Indeed, they may have exacerbated them. For Freeden, one of the great virtues of the new liberals is that they restore a whole moral ethic to what had become a very materialistic liberalism. Unfortunately, morals have a funny way of getting lost when structural, materialistic dilemmas exist.

Freeden's book should be read for two reasons. It is a scholarly study of an important group of intellectuals and activists. And it is an example of the new liberal perspective through which modern liberals will view the possibilities for saving the crisis-ridden welfare state.

MARK E. KANN

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Law and Society: An Introduction. By Lawrence M. Friedman. (Englewood Cliffs, N.J.: Prentice-Hall, 1977. Pp. xiii + 177. $7.95, cloth; $3.95, paper.)

Aimed consciously at a "general college audience," this book seeks to draw upon social science research and perspectives to examine the relationship between law and society. The guiding assumption of the work is that social forces produce or influence law. Lawrence Friedman examines a number of important questions, including the functions of law in society, its origins and historical development, the nature and variety of legal culture, the relations between social-cultural forces and the law, the reasons for compliance with the law, and the role of law in social change.

Friedman offers an exceedingly broad explicit definition of "law" and "legal system." "At the heart of the law we find authoritative rules or norms, and institutions, processes, and people that deal with them—those who make rules, or give orders to other people, or interpret rules and norms, or try to carry them out" (p. 4). As he notes in the final chapter, law used in this way "becomes a synonym for government." The "legal system" is "a working process," and consists of "behavior, movement, demand, and response" (p. 5). Frequently, the questions examined and the examples provided adhere to the broad, explicit definition of law. Legislatures' and administrative agencies' contributions to the function of social control, for example, are discussed along with those of courts. Chapter 8 ("Explaining the Law") asks why a legislature, a court, and a local planning board act as they do. The answer focuses on "social theories of law," and examines the role of public opinion, power, and interest groups.

Throughout the book, however, implicit and narrower definitions of law and the legal system focusing upon statutes, precedents, courts, lawyers, and judges provide the criteria of relevance. The author includes chapters introducing students to theories on the origins and history of law, the nature of the legal process, the types of legal systems (common law, civil law, etc.), the factors that determine compliance with law (sanctions, group interests and pressures, and morality), and the role of law in social change.

The book encounters serious problems in providing a general college audience with an introduction to the links between law and society. Its brevity (165 pages of text) compounds the inherent difficulties such an ambitious enterprise presents. When it focuses on general questions consistent with the explicit broad definitions of law, it fails to incorporate much relevant literature from political science. Chapter 8, for example, contains practically no references to research and theories on representation, or the links between public opinion, power, and interest groups on the one hand, and governmental decisions on the other. The chapters which reflect the implicit and narrower definition of law and the legal system too often ignore major social science studies published at the time the book was written. The author asserts, for example, that legal research has neglected the work of lower courts, certainly an overstatement by 1976.

Friedman poses many important questions and offers a number of insightful ideas about how to approach answering them. His use of historical and comparative data is especially impressive. Intriguing ideas (for instance, that litigiousness declines as industrialization proceeds) are sprinkled throughout the book. But this brief volume fails to present a comprehensive and well-grounded introduction to the relationship between society and law as broadly defined for introductory political science students. It is too broad and too short for students in specialized courses dealing with the legal process, courts, judges, and the like. The presentation is too condensed, and the argument too often difficult to follow. Undergraduates in the general college audience will frequently find the work baffling. Scholars interested in Friedman's intriguing ideas ought to read his earlier and more extensive work, *The Legal System: A Social Science Perspective.*

JAMES EISENSTEIN

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Philosophers of Peace and War: Kant, Clausewitz, Marx, Engels and Tolstoy. By W. B. Gallie. (Cambridge: Cambridge University Press, 1978. Pp. x + 147. £5.95, cloth.)

This is a splendid little book, penetrating, stimulating, and illuminating. Gallie rightly thinks that we can better understand our present situation by listening carefully to the theorists whom he has chosen to study. Their works do not add up to a unified theory of war and peace. Rather each offers a distinctive perspective on human conflict and what can be done about it.

Kant comes through as much more tough-minded than many have recognized. Reason demands that an end be put to war, that relations among states be legalized. But peace cannot be enforced; it cannot be achieved through empire or federation. Only voluntary agreement among all states will suffice, and that "can be expected only when war has become intolerable and indeed unthinkable" (p. 34). Kant appreciated what today we call mutual deterrence.

Gallie's treatment of Clausewitz is particularly impressive. Clausewitz has been a puzzle, because he seems to oscillate between a conception of war as a political instrument and war as absolute, the inner logic of which requires escalation to an extreme. And this, as Clausewitz well knew, rarely happened. Gallie shows that Clausewitz mistakenly assumed that a definition of war must express the most important fact about it. But war conceived as escalation to knockout does not square with its use for rational political purpose. Why then was Clausewitz's attention so riveted on escalation and not on limited forms of war? He is alerting military commanders to their supreme danger. "It is this danger—the permanent possibility rather than the calculated probability of a knockout—that, more than anything else, explains Clausewitz's obsession with Absolute War" (p. 58). This is an obsession that continues to characterize strategic thinking.

Marx and Engels were the first to attempt a sociology of war, but the ubiquity of war does not accord well with their basic theories. Pragmatically, they were inclined to view a general European war as an opportunity for their revolution. Engels, who was a close student of military history and affairs, came to know better. General war would be a calamity with incalculable consequences for the cause of socialism. Socialist parties should work to prevent it by coming forward as nonaggressive parties of national defense. In actuality, Lenin thought otherwise. "For, contrary to Marx and Engels, who had maintained that Tsarism would fall only to a revolution from within, Lenin was persuaded that it would fall only as a result of an utterly disastrous war—and that meant as long a war, and as generalized or global a war, as possible" (p. 98). Lenin was quite prepared, Gallie argues, to sacrifice international socialism for the sake of his revolution in Russia.

Tolstoy's thinking about war distilled into the notion that "bureaucratic" or "political militarism" is the supreme evil of human society. "War is only the last climax of an infection that runs through every political society" (p. 131). All states, of necessity, degenerate toward oppression at home and aggression abroad. The Tsarist autocracy is only an extreme instance of this political truth. People, now hypnotized by patriotism, as individuals must offer resistance to militaristic habit and impulse. This is the only hope for freedom and peace.

Gallie proceeds to a confrontation of his philosophers. He confronts Kantian rationalism and Tolstoy's moralism with Clausewitz's conviction that war is simply inherent in the "competitive life of peoples." "No doubt both Kant and Tolstoy would have admitted that this was true: but they did not recognize, or did not sufficiently emphasize, how much it must affect the seeming simplicity of their conclusions" (pp. 135–36). As for the Marxists, "Kant is to hand with the powerful claim that they have simply failed to recognize the distinctive and permanent character of the international problems—namely that conflict of interest will always arise between communities that acknowledge no common political authority" (p. 137). Gallie points to the new military technologies and says: "They have falsified Clausewitz's claim that, as between comparable adversaries, the most logical way of fighting, for either side, is to escalate or maximize one's efforts with a view to achieving absolute victory, or at least to threatening one's adversary with absolute defeat" (pp. 138–39). We have moved from the strategic doctrine of mutual assured destruction to that of flexible response.

Gallie concludes by calling upon political philosophers to carry through Kant's project of analyzing the nature of international relations governed by Reason. Political scientists should undertake systematic analysis of the use of force to reveal those points "where competition (which is surely endemic in all life) passes into conflict and coercion, and where coercion passes into intolerable oppression and war" (p. 141).

All I should wish to add is that some of us continue to be concerned with types of strategic thinking and their applicability within the evolving structure of world politics. We live in circumstances uncontemplated by our philosophers of peace and war, in which we have to deal with ruined civilizations whose weakness pushes them toward military adventure. If Kant's Reason is ever to prevail, we shall have to think hard—and keep our powder dry.

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Law, Morality, and Society: Essays in Honour of H. L. A. Hart. Edited by P. M. S. Hacker and J. Raz. (Oxford: Clarendon Press, 1977. Pp. vii + 312. $16.50.)

The editors of this *Festschrift* claim that H. L. A. Hart has done for legal philosophy in the twentieth century what Bentham did for jurisprudence in the eighteenth century. That the clarity of their distinction and the evidence for their claim remain debatable should not detract from the merits of the 16 contributions from "colleagues, former pupils and devotees."

The contributions range from those in which authors pursue their own interests independently of Hart's ideas to those which explicitly deal with them. Examples of the first group are B. M. Barry, "Justice Between Generations" and J. Feinberg, "Harm and Self-Interest." Some, such as R. M. Dworkin, who is again in quest of right answers, can also be interpreted as making an implicit critique of Hart's ideas, as G. M. Marshall so reads Dworkin in criticizing him. Some others, such as J. Raz, "Promises and Obligations," can also be seen as attempted extensions of some of Hart's ideas.

In the second group are those who mainly explicate or critique Hart's ideas. It is these essays which make the work as a whole worth careful reading. P. M. S. Hacker presents a useful exposition of key ideas in Hartian legal philosophy. That he is sympathetic to Hart's approach makes his occasional criticism more telling. Of special interest is Hacker's conclusion that Hart's "rule of recognition" has reduced to an untidy picture the positive concept of the unity of a legal system as pyramidal (p. 24). But he leaves one wondering how Hart's more realistic, fragmented pyramid of rules and norms satisfies the need of judges and others for criteria by which sharply to distinguish law from morality.

The essays by J. R. Lucas and P. R. Foot impart stimulating insight into the limits of Hartian legal philosophy. Lucas picks up where Hacker left off and explains why the "rule of recognition" must necessarily leave an untidy picture of the unity of a legal system. The reason is that law in society depends more on morality than the positivist picture can be stretched to accommodate, seemingly not even by Hart's "intersection of law and morality."

It is because Hart saw that his *The Concept of Law* may "be regarded as an essay in descriptive sociology" (*Concept of Law,* p. vii) that Foot's essay has special, though implicit, relevance to the assessment of Hart's ideas. Foot argues that morality is essentially a social phenomenon, and not the product of moral philosophy created, Cartesian style, by single individuals transcending society. If that is so, what is left of Hart's key argument against Lon Fuller that in the face of morally iniquitous laws we are morally free or obliged to criticize and even disobey them, but not to deny morally their validity as law?

A. M. Honoré also engages in descriptive sociology in his essay on "Real Laws." Using Raz's essay as a counterpoint, he exposes the futility of a Benthamite quest for the "real entities" behind observable laws. He finds that "the world of real laws is both more rational and more reassuring than the Benthamite maze" (p. 118), all of which appears to leave in basic doubt the value of analytical jurisprudence as "the clarification of the general framework of legal thought" (*Concept of Law,* p. vii).

R. S. Summers is more explicit in taking Hart to task, along with Bentham and Austin, for neglecting the social side of law by engaging in "naive instrumentalism." For Summers, not even the untidy picture of the positivist pyramid of rules appears acceptable (p. 121). One can escape "naive instrumentalism" only by stressing sociological over analytical concerns (p. 131). But, in his call for this new orientation of legal realism he leaves us this picture of law: a diffuse process employing many implementative devices (at least five) beyond coercive sanctions in serving multiple goals on three levels, the unanticipated consequences of which activity may be more significant than the manifest ones. At the least, Summers' essay forces one to reflect on whether a "sophisticated instrumentalist" must necessarily be a "naive conceptualist."

The remaining six essays are also of commendable quality. Space, however, only permits comment on D. N. MacCormick, "Rights in Legislation," in which a rights theory fundamentally at variance with Hart's is developed by Hart's techniques. That attitude reflects the value of the work as a whole. Wisely, in the main the contributors to this *Festschrift* truly honored Hart by disregarding the claim of the editors as to his purported Bentham-like influence, and instead by regarding his ideas as worthy of the best critical analysis.

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Socialism and Bureaucracy. By Andras Hegedus. (New York: St. Martin's Press, 1976. Pp. 193. $14.95.)

The Structure of Socialist Society. By Andras Hegedus. (New York: St. Martin's Press, 1977. Pp. 229. $9.95.)

The Humanization of Socialism: Writings of the Budapest School. By Andras Hegedus, Agnes Heller, Maria Markus, and Mihaly Vajda. (New York: St. Martin's Press, 1976. Pp. 177. $14.95.)

The Budapest "school of socialism" traces its origins to Georg Lukàcs' encouragement that "works like these are the forerunners of the philosophical literature of the future" (his letter published June 11, 1976, in *The Times Literary Supplement*). Hungarian Communist leadership, however, has disputed the grand old man's sponsorship of the rebel Marxists. Actually, they wrote under the leadership of Andras Hegedus, Hungary's last Stalinist premier, who at the time directed Sociological Research Group of the Academy of Sciences. Hegedus presided over the government during the twentieth Soviet party congress. Both this congress and the Hungarian Revolution inspired him to seek pragmatic though essential reform of the system. His book on the structure of socialist society draws largely on empirical research data which his institute had collected in selected rural areas. Historical irony thus would have it that a Stalinist leader would sponsor the New Left intellectual circle that the Budapest school was. Elements of New Left radicalism have been discovered in Soviet Estonia (their linguistic affinity to the Hungarians most likely has nothing to do with it), but the Hungarians are the main exponents of the New Left in Eastern Europe.

Many though by no means all of the writings here reviewed were actually published in the reform-minded Hungary of the 1960s. Their authors, however, are all "fallen" Communists. Hegedus protested Soviet invasion of Czechoslovakia and at first lost only his position, although in 1973, together with Mihaly Vajda, he was expelled from the Hungarian Socialist Workers party. Agnes Heller, a student of Lukàcs, has lost party membership already in 1959 while Maria Markus, Hegedus' frequent co-author, was expelled in 1968. In 1977, the group was found guilty by special Central Committee investigators of an illegitimate attempt to formulate a neo-leftist alternative to Marxism-Leninism. Not to be subdued, in 1977, though with the exception of Hegedus, they signed a statement of solidarity with the signers of the Czechoslovak Charter 77.

What is the neo-leftist alternative to orthodox Communism that these dissident authors propose? Hegedus and his colleagues press the view that an already advanced socialist society should discard a dogmatic ideological doctrine ("histmat") and with the help of free, empirical, though value-oriented, social sciences should engage in self-analysis. Such social analysis, not considerations of power which were important in the earlier day, would compel a progressive development of socialism characterized by optimization and humanization. Negative characteristics of society, too, could then be corrected without ideological inhibitions. Hegedus finds such characteristics not only in the city but also in the village where old differences have been replaced by the new ones based on prestige and occupation. The Hungarian villager has not yet been transformed from a peasant into a worker and Hegedus, constantly referring to Soviet experience, does not seem to be in a hurry to push it by merely ideological or propagandistic means. He also shows that the village family structure, including a number of traditional attitudes, remains strong.

The main enemy of socialism, according to him, is the bureaucracy, ruling for its own selfish ends, as it does in Eastern Europe. Not denying the need of management, Hegedus opposes its overlapping with political structure and insists not on political, but on social (Heller even voices belief in direct democracy) supervision of economic and social life. This of course pushes the Communist party to the margins of society by denying its function as a leading force. The authors of this school similarly oppose the centralizing characteristics of socialism. They would have society run by communities of self-management. The Hungarians however, would go farther than the Yugoslavs, although it is not clear how far.

In *Humanization of Socialism*, Agnes Heller advocates a total revolutionizing of life, not just the abolition of private property. In an essay co-authored with Mihaly Vajda, she foresees the disappearance of monogomous family. In other essays, she sees changes of "specific" sex morality (there is nothing specific about sex) and advocates a collective family as an aspect of society consisting of "organic" communes which would articulate needs arising from everyday life. Markus and Hegedus criticize the Marxist-Leninist "myth of collectivity" as not providing answers to Socialist development. End of private ownership did not produce workers' power of control; it was instead put into bureaucratic hands. Wage labor, too, has essentially survived. But the Hungarian, and generally, East European society is capable of overcoming these deficiencies through the "growth of communal forms." Markus further tackles the problem of free time suggesting that it should not be turned into a commodity (money making) but be used for "overtraining" so people actually acquire a choice of occupations in the economic system.

The "Budapest school" thus is dominated by the conviction that East European socialism is capable of optimalist and humanistic development from within, through a certain inevitable development of social forces helped by an unfettered sociological although Socialist-oriented analysis. In the middle of the 1970s, however, its main spokesman, Andras Hegedus, saw his hopes for renewal through such social change destroyed. In a very revealing commentary on the consequences of the twentieth Soviet party congress, he confessed to "social pessimism" and seemed to conclude that change toward a liberal society in Eastern Europe will come not through the discoveries of a critical "Marxist social science," though these will help, but through a change of the structure of power.

This admission really shows the impotence of the group's theoretical approach. While the writers' commitment to the early Marx's humanism and to critical social study is admirable, neither Heller's enthusiasm for Che Guevara nor Hegedus' and others' confidence in the intellectual genius of C. Wright Mills offers a viable alternative. Theoretically, after Djilas, Kolakowski, Marcuse, Garaudy, Medvedev and others, these essays do not offer much new, if anything, except the yearning to rejoin the secular liberal Western society. This yearning, however, is flawed by an almost dogmatic insistence that any "optimized" and humanized life can begin only with the abolition of private property and possession. These authors really belong to Reinhold Niebuhr's category of children of light who pursue a mere illusion, not a reality, of social harmony and perfection, because they refuse to recognize that it is not only private but also collective property and possession that can distort society and hurt the individual; the central question in the pursuit of perfection is not ownership but power.

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The Social Thought of Bernard Mandeville: Virtue and Commerce in Early Eighteenth-Century England. Thomas A. Horne. (New York: Columbia University Press, 1978. Pp. xii + 123. $12.50.)

When a contemporary wrote of Mandeville's ideas that they "contradict the sense of mankind in all preceding ages," that "so strange an assertion makes all writers of politics, from Plato to Machiavelli, contemptible ignorant fools" (p. 80), he was singling out for ridicule the infamous doctrine of "private vices, public virtues." With these few words, with poetry, and with occasional essays and tracts Bernard Mandeville shocked the pious and the learned in the first three decades of eighteenth-century London. His work appeared as a sudden, cold shock of cynicism and realism. His writings called into question centuries-old doctrines of civic humanism which assumed that society was held together by active promotion of the public good, that virtuous public-spirited citizens were essential for the good polity. On the contrary, wrote Mandeville, the avaricious, vain and self-interested individual produces public good, social progress and national wealth. The revolution in social ethics that would usher in the bourgeois age was rendered in Mandeville's poetic couplets:

> Then leave complaints: Fools only strive
> To make a great an honest hive.
> T'enjoy the worlds conveniences,
> Be famed in war, yet live in ease,
> Without great vices, is a vain
> Eutopia seated in the brain.
> Fraud, luxury and pride must live,
> While we the benefits receive. . . .

Mandeville, the author of this poetic *Fable of the Bees,* has for too long been left to literary scholarship and occasional pieces by worldly economists. However, his importance for social and political thought has finally received book-length treatment. Thomas Horne has written a splendid little book. It is learned, thoughtful, well written, and extremely useful. I can think of no better work to recommend to students who want to learn about the incredible Mr. Mandeville (what other political philosopher, after all, wrote a treatise in defense of legalized brothels?) and his central place in eighteenth-century moral controversy.

Horne's treatment of Mandeville is a model of scholarly economy. The first chapter describes the moral climate, or should one say the moral crusade waged by the Society to Reform Manners, against which emerges Mandeville's defense of self-interest and a realistic ethic. Here Horne sets forth the tension between, on the one hand, Mandeville the theorist of selfishness and commerce, and, on the other, the received political wisdom of virtue, public spirit and, of course, frugality and disdain for money. A second chapter follows tracing the roots of Mandeville's "pessimism" to their French sources, Jansenism, and the writings of Rouchefoucald, Fontaine and Bayle. A chapter on Shaftesbury provides Horne an opportunity to contrast Mandeville's ideas with those of this great moralist champion "of an aristocratic view of the world, which denies the essentially egoistic and prideful nature of man" (p. 50). Particularly valuable in this chapter is the discussion of Mandeville's distinction between self-love and self-liking with its anticipation by several decades of Rousseau's contrast of *amour de soi* with *amour propre.*

The heart of this fine book is in its discussion of "Mandeville and mercantilism," where Horne takes the reader into the intricacies of early eighteenth-century economic history and doctrine and shows how Mandeville's writings so accurately mirrored profound shifts in public and elite attitudes. Here the reader learns a good deal about the debate on luxury, about Mandeville's arguments against educating the poor, and most importantly, about the myth of Mandeville as theorist of laissez-faire. A final chapter traces Mandeville's influence on the writings of Francis Hutcheson and David Hume, who, while very much in his debt, transcend Mandeville in their reconciliation of virtue and self-interest. Hume, indeed, goes so far as to suggest in his essays that commercial society and the spread of luxury contribute to moral progress.

Mandeville is by no means a social theorist on the order of Hume or Smith, but now because of Horne's excellent book, we know better that these more well-known thinkers did not emerge out of nowhere. In the background, having aroused the wrath of most and the interest of some, are the poetry and polemics of the Dutchman-turned-Englishman (what a marriage of the commercial spirit!), Bernard Mandeville.

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The Marxian Legacy. By Dick Howard. (New York: Urizen Books, 1978. Pp. xv + 340. $15.00, cloth; $6.95, paper.)

In summarizing this lengthy and dense book, I distinguish between two types of contributions that it makes. The first is the scholarly study of the thought of eight writers whose names adorn the book's cover: Luxemburg, Bloch, Horkheimer, Habermas, Sartre, Merleau-Ponty, Lefort, and Castoriadis. The second is the author's theoretical reflections on what he regards as the disappointing outcome of the recent encounter between Marxist theory and New Left practice; these are epitomized by the detail from Pieter Bruegel's painting, "The Blind Leading the Blind," which appears on the same cover.

Dick Howard has already established a reputation for himself as a scholar, and students interested in any or all of the theorists whom he discusses are bound to profit from some of his summaries and from his often extensive footnotes. He subordinates these contributions to his own theoretical objectives, however, and the effect of this on the reader is a feeling of frustration. For example, the treatment of Rosa Luxemburg focuses on the question-stopping idea that, brilliant as she may appear, she was *"refuted by History"* (p. 59). Different but comparably negative treatments are accorded to several others. In the case of each theorist whom he studies, Howard selects one or a few writings as central for his purposes, often disregarding what the authors' own principal purposes actually were. The result is that none of his scholarly chapters, with the possible exception of that on Castoriadis with which the book abruptly ends, is really systematic or comprehensive. And Howard's presentation of Castoriadis' obscurantist technical vocabulary is not likely to inspire instant agreement with the author's very positive view of him.

What, then, *are* Howard's theoretical goals? Unfortunately, that is not easy to say. The message of disillusionment conveyed by the Bruegel painting and by many of Howard's remarks is obvious enough: his earlier assumption that Marxism provided the appropriate framework for understanding the movements of the 1960s "was mistaken" (p. x); "the incoherence of the New Left is astounding" and "it died so pitifully" (pp. 19–20); Marx was "led astray" by his ontological presuppositions; Marxism is "a metaphysics," "a tautology," and "turns out to be based precisely on premises which make the realization of its self-defined goal impossible" (pp. 284–85). On the other hand, Howard constantly insists that "we cannot think without *Marx*" (p. xiii); at one point (p. 64) identifies himself with "engaged Marxists"; speaks glowingly (p. 153) of the May 1968 uprising in Paris; and asks how "Lefort's contribution . . . fits into the framework of a New Left politics for which we are reaching" (p. 227). Howard has become greatly attracted to the later thought of Merleau-Ponty, who was also a seminal influence on the "Socialisme ou Barbarie" group, in which Lefort and Castoriadis were active. Merleau-Ponty sometimes designated his quasi-relativistic, anti-dogmatic, anti-transcendental stance as a "non-philosophy," a type of thought that is at home with ambiguity, uncertainty, inconclusiveness. Apart from the fundamental question of its merits, it is a difficult stance to sustain, over many pages, without causing the author to appear at times hopelessly vague and incoherent, even self-contradictory.

Howard has not avoided these dangers. His leitmotif is a call for a return to "the political," which he accuses the New Left, Habermas, Sartre, and ultimately Marx himself of having neglected. He admits to having been criticized for using this concept in a way that is "either too vague or worse" (p. xii). The criticism is justified.

In part, his accusation that radical theorists have neglected "the political" seems to stem from Howard's acceptance, together with Lefort and Castoriadis, of a rigid and, in my opinion, erroneous interpretation of Marx's views about the dominance of economic factors and the predictability of capitalism's collapse—an interpretation that has been taken over from the Soviet version of Marxism. But perhaps a more fundamental problem lies in Howard's stylistic reliance on the coupling of highly abstract nouns with definite articles—"the political," "the social," "the theoretical," etc.—to attempt to convey meanings which are not at all perspicuous to readers. It is a *damnosa haereditas* from some of the French writers whom he adulates.

With his difficult style, his tendency often to presuppose a prior knowledge of various theoretical positions to which he alludes, and his well-warranted admission (p. xiv) that "no recipes are forthcoming here," Howard demands a great deal of his readers. However, *The Marxian Legacy* is still a very substantial and interesting document about some of the transformations of New Left theory, written by one of the better-informed commentators on the topic.

WILLIAM LEON MCBRIDE

Purdue University

Political Society: A Macrosociology of Politics. By Edward W. Lehman. (New York: Columbia University Press, 1977. Pp. xi + 247. $15.00.)

Economists have developed reasonably well-defined distinctions between micro-economics and macro-economics. Unfortunately, political scientists have not given sufficient attention to the "micro-macro" distinction and its importance in explanation. Several years ago Heinz Eulau provided us with a thoughtful discussion of the problems of levels of analysis in *Micro-Macro Political Analysis* (Chicago: Aldine, 1969):

> The analyst who moves toward the micro pole adds empirical data that make for greater validity and reliability but he possibly sacrifices significance . . . the analyst who moves toward the macro pole deprives himself of data, loses in validity and reliability, but possibly gains in significance (p. 18).

But the movement toward reductionism—the tendency to lapse into more microscopic and subinstitutional directions—continues unabated in political science.

Now a sociologist, Edward W. Lehman, tells us that states, parties, corporations, ethnic groups, the mass media, and other macrosocial groupings are the "crucial elements" in shaping our political life. According to Lehman, these whole political entities should be studied not only for the purpose of explaining politics, but also for the purpose of *praxis*—finding out how to bring about social change. Lehman is clearly writing an essay for the 1960's generation of would-be social reformers who cannot quite understand "what went wrong" with their movement and why. The answer, according to Lehman, was their failure to appreciate the full extent of "macropower" and to understand "the separation of systemic power from inter-member power" (p. 80).

To understand society, as well as to transform it, we must gain an understanding of "macropower." Lehman's definition of macropower is closer to Max Weber's classical definition of power—emphasizing capacity, potential, symbolism, and universality—than it is to Robert Dahl's definition of power—emphasizing interpersonal, limited, and specific processes. Lehman distinguishes between the "macropower" of large institutions in society, and the "micro-power" exercised by individuals in decision-making situations. The macropower of large institutions depends upon not only coercive actions but also upon reputations, symbols, norms, access to information, and related resources. Lehman *says* he avoids "the so-called

Power Elite debate," but, of course, he has entered the fray as a potent critique of the "micropower" approach of the pluralists.

Having described the concept of "macropower" and the "political culture" in which it thrives, the author proceeds to describe, first, "the morphology of intermember power" (local groups, ethnic groups, secondary associations); and then "the morphology of political power." This latter topic includes the "macropower" of the state, the corporation, social classes, parties, elite structures, and so on. These descriptive chapters are largely devoid of any empirical references; political scientists may find them somewhat superficial and disorganized.

The book concludes with a warning to potential revolutionaries regarding "the longevity of macrosocial forces" (p. 189). Indeed, the author even acknowledges the existence of the "neo-conservatives," Daniel Bell, Irving Kristol, Nathan Glazer, and Robert Nisbet, who are "fundamentally pessimistic" about transforming society. But Lehman himself is optimistic about the "malleability" of macro levels of power, although he never says why. Indeed, his view of "macropower" seems to contradict his conclusion that "change is possible." We learn from Lehman that we must grasp the *macrosocial* levers of change. Unfortunately, Lehman never tells his readers how to grab hold of these levers.

The value of this book is that it warns us against the reductionism in social science which threatens to render our work both irrelevant for society and unable to explian major social forces of our time. Power in America is organized into large institutions. Both political scientists and sociologists must undertake the task of systematically examining these institutions as whole political units. "Anyone concerned with changing society—via direct action or policy analysis—is advised to look for levers in the macrosocial domain" (p. 11).

THOMAS R. DYE

Florida State University

Political Identity. By W. J. M. Mackenzie. (New York: St. Martin's Press, 1978. Pp. 185. $13.95.)

If you want a knowledgable, insightful, skillful exploration of the variety of uses and abuses of the notion of "political identity" in political theory and in contemporary social science, this book will delight you. If your idea of science involves the stripping of ambiguity from key concepts and a focus on techniques for verification of propositions, it will bother you, and that will be all to the good. Erik Erikson, the popularizer of the term "identity" in recent times, wrote in his book, *Identity: Youth and Crisis,* that "the more one writes about this subject, the more the word becomes a term for something as unfathomable as it is all-pervasive. One can only explore it by establishing its indispensability in various contexts" (p. 9).

W. J. M. Mackenzie teaches us a great deal about political actions, political language, political analysis, and political justification by examining the range of issues important writers have dealt with under the rubric of "identity" and the wide range of assumptions in social science and social criticism about psychological, social, territorial, genetic, and other bases of identity. He is enlightening not only about the linguistic contexts in which the term has been used, but also about logical and empirical links among language, social conditions, and theoretical orientations.

Among the scholars and social critics whose work on identity Mackenzie considers are: Lucian Pye, Erik Erikson, Erich Fromm, Herbert Marcuse, Jean Paul Sartre, Albert Camus, Erving Goffman, Arthur Bentley, and Mancur Olson. From one perspective his observations look diffuse. But they are so only because the antecedents and the component elements of thought are diverse and operate concurrently at different levels of abstraction and in different modes of discourse, forming cognitive structures that are never neat or static. Mackenzie is deft at exploring such complexity and its implications for the shaping and the acceptability of social theory. He examines not only what these writers have to say about political identity but also the social conditions and the intellectual currents that give them their appeal; and he often presents his reasons for regarding their contributions as valuable or as unfortunate.

In a chapter entitled "Powerful Abstractions," there is a perceptive essay on the ambiguities of the concepts of nation, race, religion, and class as contributors to political identity and as explanations for it. Another chapter deals with modes of expression: language, myth, symbol, ritual, and ideology.

Mackenzie is impressive as critic, interpreter, and synthesizer. In playing these roles well he provokes his readers to play them too.

MURRAY EDELMAN

University of Wisconsin, Madison

The Structure of Marx's World-View. By John McMurtry. (Princeton, N.J.: Princeton University Press, 1978. Pp. xii + 269. $15.00, cloth; $3.95, paper.)

Marx's thought, according to John McMurtry, provides "the most effective witness" to "the human condition." This oddly phrased, inflated claim "is best testified to by the conversion of half the globe to its guidance in the last sixty years." The status of this testimony and the nature of this "guidance" do not concern McMurtry. They belong "mainly to the world outside the Western Academy," while *The Structure of Marx's World-View* sets its sights, and belongs, firmly within it. What bothers McMurtry is not whatever might account for the effectiveness of Marxism as a world-historical doctrine, but the dismissal of Marx's thought by "the mainstream of Anglo-Saxon philosophical thought as unworthy of serious attention." Those who inhabit this "mainstream" believe for various reasons that Marx was confused; McMurtry, going through these reasons systematically in order to clear the ground and to accomplish "philosophy's classic task of underlabourer" is more ambitious than this modest Lockean phrase suggests. Marx's work, McMurtry assures us, "demands systematic clarification . . . a step-by-step reconstruction of its categorical framework, its determining relationships, and, at bottom, its concept of human nature. . . . We undertake to distill from the enormous and perplexing range of his corpus a clear and integrating structure, which is both propositionally lucid and faithful to [Marx's] writings."

McMurtry is better at distillation than reconstruction. Rather than venturing "into the details of this massive literature" of Marx's, "we identify the set of principles that are of general importance" so that "Marx's stance on the most basic philosophical issues" can be pinpointed and defended. If propositional lucidity and textual investigation conflict (as they do), McMurtry, whose Marx is tailored to the requirements of his "mainstream," opts for the former. Orderliness (which McMurtry appears to value for its own sake) is the strength as well as the weakness of his book. McMurtry is part "underlaborer" clearing away the debris deposited by the "mainstream" (and apparently by no one else), and part prospector, extracting from the mother-lode of Marx's "corpus" propositions which, when suitably juxtaposed and mounted in display cases, add up to a coherent "world view" or "philosophy of the human condition." That Marx, for his part, was unconcerned even to *address* issues of this kind, which McMurtry at times credits him with having *solved,* does not deter our intrepid prospector from high-handedly claiming that he can "ascertain the Logos of Karl Marx's vision [which] is, if he sees aright, to ascertain the Logos of our own world."

The value of McMurtry's book has nothing to do with such inflated and ahistorical claims; before his Lockean modesty deserts him, McMurtry makes some good points. His procedure is most defensible where it is most central. From Marx's "Preface" to the 1859 *Contribution to the Critique of Political Economy* he extracts a sequence of "essential categories": forces of production; relations of production; legal and political superstructure; ideology; and forms of social consciousness. These, sandwiched between a chapter on Marx's conception of human nature and two concluding chapters on economic and technological determinism, become the subjects of the five core chapters of the book. In elucidating their meaning and drawing out their implications, McMurtry is at his ablest and his book at its strongest. As textual scholarship it cannot hold a candle to the obvious comparison, Terrell Carver's *Karl Marx: Texts on Method* (a work which locates Marx in his own time and place, which McMurtry never sees the need to do); but it does provide a serious, honest attempt to examine and clarify some of the categories of Marxian analysis. The attempt is useful and by no means recondite; it deserves to be read by anyone who would so much as discuss the categories in question.

If only McMurtry had left it at this. Unhappily, he adds a good deal of bombast that fails to help or sustain his argument. Some of his claims, made in passing, are astonishing in their ambitiousness: that "Technology . . . is Marx's Providence" (much of the evidence McMurtry adduces would point away from so glib a conclusion); that Marx had a justifiable theory of mind; that his mature writings are regulated by a system of correspondences that need not be explained dialectically; that some scientific laws in Marx's "corpus" are sufficiently falsifiable to satisfy Popper (is Popper listening?); and that Marx's determinism is compatible in a sense with free will. In advancing claims like these—the list is not exhaustive—McMurtry both spreads his arguments too thinly and makes it systematically impossible for *any* reader to agree with everything he says. One can only hope that McMurtry's overambitiousness will not deter the reader from paying attention to what remains worthy and valuable in his book.

PAUL THOMAS

University of California, Berkeley

The Old Religion in the Brave New World: Reflections on the Relation Between Christendom and the Republic. By Sidney E. Mead. (Berkeley: University of California Press, 1977. Pp. xii + 189. $10.00.)

The question with which this book is concerned is why so many American church members seem to have "theologically bifurcated minds," which refers to their simultaneous adherence both to the traditional orthodoxies of "sectarian" Christianity and to the "Enlightenment" perspective on which this republic's founding was premised. The problem is that the two are incompatible and "the theology of the denominations does not legitimate the political and legal structure of the commonwealth" (p. 69).

A student of American church history, Sidney Mead provides an historical account of how this came about. Based on Mead's Jefferson Memorial Lecture at Berkeley, this work focuses on what most made the eighteenth-century American religious environment different from the environment previously prevalent in Christendom, namely the novel American experience of religious pluralism. From this fact of pluralism proceeded, as a practical necessity, the federal Constitution's "territorial solution" prohibiting a national religious establishment but leaving states free to maintain established churches as they chose, as some did. Mead ably illuminates the historical process by which the fact of religious pluralism contributed to the eventual triumph of the "Enlightenment" perspective. He also shows, however, how an acceptance of an "Enlightenment" attitude ultimately requires a radical transformation of religion itself. Indeed, since he himself seems to share that "Enlightenment" stance, a critique of conventional American religion pervades this book.

Political theory has provided those wishing to make religion more suitably serviceable to society with two possible models—Machiavelli's and Hobbes'. Seeing little political potential in Christianity itself, Machievelli praised pre-Christian Roman religion as an eminently effective civil religion. Accepting Christianity's importance in his culture, Hobbes in contrast concentrated on so transforming it that it could become completely compatible with the premises on which the Leviathan-commonwealth would be based. Machiavelli is nowhere mentioned in this book, and Hobbes appears only peripherally in another context, but Mead's approach can best be categorized as of the Hobbesian variety. Thus, he finds "that the divine agency in the world, the essence of Christianity, is persuasion and not coercion" and that "those who led the movement to incarnate in civil institutions the ideal of persuasion (government by the consent of the governed; Tocqueville's dogma of the sovereignty of the people), were in their day more representative of Christianity than were the orthodox defenders of Christendom's established churches, and those who tried to make Christendom's species of institutionalized religiosity part of the Common Law of the land" (pp. 7–8).

Even more startling, however, is how Mead transforms original sin. He recognizes that the issue in Eden was moral independence and sees it as "the mythological statement of the origin of the principle of consent" (p. 38). This principle he sees as having been "explicit in early Christianity" and expressed in *Acts* 5:29, "we ought to obey God rather than man" (pp. 38–39). Just how that injunction really relates to "consent" is, to say the least, unclear. How the sin of pride manifested in an act of disobedience against God would be "the mythological statement of the origin of the principle of consent" is quite conceivable, but how it could then also be expressed in an injunction to obey God is not. In any case, the fundamental fact, barely alluded to in this attempt to show that the "Enlightenment" perspective is more Christian than that of orthodox Christianity, is that both *Genesis* itself and early Christianity clearly condemned Adam's sin.

One of the most intriguing aspects of this work is surely the contrast drawn between the religious perspective of such seventeenth-century Puritans as John Winthrop and John Cotton and that of eighteenth-century figures like Jonathan Edwards. Because of the theological perspective that pervades this book, only the obvious desire for a suitable civil religion explains Mead's fascination for the theocratic ideal of the seventeenth-century Puritans and his criticism "that the internalization of religion beginning with the eighteenth-century revivals effectively separated assurance of salvation from a sense of responsibility for the institutions of the convert's society" (p. 69). Surely, one would think, the Puritan position presents an even greater threat than "the internalization of religion" does to the secular society premised upon the "Enlightenment." The Puritan ideal, after all, entailed creating a society modeled upon religion, not the adaptation of religion to the theoretical underpinnings of a secular society. The author's transformation of Christianity into the philosophy of the "Enlightenment," by evacuating its troublesome theology, enables him, however, to praise the Puritan ideal in the radically tamed form found, finally, in the thought of Lyman Beecher.

RONALD FRANCO

Marquette University

Escape from Predicament: Neo-Confucianism and China's Evolving Political Culture. By Thomas A. Metzger. (New York: Columbia University Press, 1977. Pp. x + 303. $15.00.)

Escape from Predicament is an ambitious attempt to correct the dominant theories about medieval Confucian thought. Its major positive theses are hard to ferret out in the introduction. They are initially presented through a contrast with standing interpretive views which are typically adumbrated in the main exposition to one or two core terms used by his protagonists. The reader must constantly go to the lengthy chapter notes to see how Metzger construes the alternative theories.

Metzger takes the erroneous elements of the received view to be mainly an undue stress on the radical discontinuity between traditional Chinese thought and modern (post-Western) Chinese thought. Metzger argues that modern thought, both contemporary neo-Confucianism and Maoism, develop out of key concerns of the Sung and Ming Confucian intellectual tradition. This link, he says, had been ignored by both humanist and behavioral accounts of Chinese thought.

Some of the main protagonists are (1) Weber, who held that Confucianism was stagnant primarily because it had no radical sense of evil, (2) Pye-Solomen who hold that Confucianism fostered dependence, (3) and T'ang Chun-i (though he presents his philosophical view in great detail) for distorting his own tradition by ignoring the sense of predicament.

I will try to spell out Metzger's title thesis, and its role in these disputes. His description of the sense of predicament takes up nearly half the book, so there is some risk of oversimplifying. But the following seem to be the key beliefs which make up this sense of predicament and the escape from same.

(A) Neo-Confucians adopt Mencius' theory of an innate morality.

(B) They interpret the Mencian moral psychology via Buddhist metaphysics—a universal mind.

(C) These views yield (with considerable scholastic detail) the "perception" that if we can "realize" our innate morality we "participate" in wielding the "creative" power of the universe.

(D) Thus moral omniscience translates into omnipotence (as long as we conform to cosmic principles).

(E) However, the neo-Confucian philosopher must notice that although Confucianism is the dominant political ideology in a dynasty with sufficient power to effect Confucian reforms, no utopia has emerged.

(F) Hence either there is no innate morality or *there must be some difficulty in actualizing* the innate and hence of achieving the moral linkage with this cosmic mind (power)—this difficulty is the neo-Confucian version of evil.

1. Achieving the linkage depends on some form of intellectual grasping of the nature of the link. (Metzger does not stress enough the Buddhist character of this assumption—evil for the neo-Confucian, as for the Buddhist, is translated into a form of ignorance or delusion—usually called "clouding" or "obscuring" in the neo-Confucian theories.)
2. The elusive definitive characterization of the differences between Wang Yang-ming and Chu Hsi lie in their formulations of the nature of the link.

(G) The impact of Western philosophy with its scientific rationality offers an account of the relation of understanding and power and resurrects the possibility of wielding moral power over nature. This results in a surge of optimism in latter-day neo-Confucians and in Maoists—the escape from predicament.

Overall this is an intelligent and important book. My major reservations center on the methodological theory which occupies a manifestly central position. The central idea of the methodology is routine enough—we should distinguish what is taken for granted within a school and what is disputed. But the point is carelessly formulated. Presuppositions of quite different logical types are all lumped together and called "perceptions" and only the disputed claims are "claims." Even more misleading is the philosophically naive assumption that we can characterize the states of mind of Chinese thinkers from centuries past using this technique while ignoring questions about the meaning, truth, and theoretical role of the sentences which fall in both categories—concerns which Metzger castigates as "philosophical." (He assures us that all his translations are "exact." We, no doubt, could confirm this if we had some such direct access to those states of mind.) The same unfortunate philosophical naivete vitiates his treatment of many of his major themes which have philosophical content, e.g., individualism, rationality, morality, and autonomy. These are minor but persistent annoyances in an otherwise superb survey of neo-Confucian thought.

CHAD HANSEN

University of Vermont

Marxism and Politics. By Ralph Miliband. (New York: Oxford University Press, 1977. Pp. 198. $10.00, cloth; $2.95, paper.)

For the better part of a century, Marx's theory of the state could only be viewed through the haze of Marx's own partial and disconnected treatment of politics and the contradictory practices and concurrent rationalizations of assorted Marxist parties. Recent scholarship by Shlomo Avineri, Richard Hunt, Hal Draper, Nicos Poulantzas and Ralph Miliband have raised the discussion to a level befitting the importance of the subject. In his latest contribution to this literature, *Marxism and Politics,* Ralph Miliband offers a popular overview of the character and role of the state in capitalist society (generally the staple of this kind of work) and of the politics that socialists do and should practice in both capitalist and post-capitalist societies. Of the two, the discussion of socialist politics is a model of scholarly insight, critical balance and revolutionary good sense.

In particular, what Miliband says about the relation of class and party, the contradiction in the notion of party (and in the Dictatorship of the Proletariat itself) between the imperatives of democracy and efficiency, and his restatement of the revolution/reform dichotomy as one of insurrection and constitutionality, deserves the attention of every serious student of this subject. I also found Miliband's scenario of what is likely to happen if (when?) a reformist Marxist party gets into power in an advanced capitalist society (Italy? France?) to be the most convincing of the dozens which are now circulating. In all these matters—and more—the cool and careful gaze that Miliband brings to an area of Marxist politics that is generally dominated by sectarian squabbling merits our deep respect and thanks.

Unfortunately, Miliband's analysis of the capitalist state proper—despite insightful digressions on the role of intellectuals and on the national question—is much less successful. Miliband is aware that at the core of Marx's theory of the state is the view that the state serves capitalist class interests through (1) members of this class who occupy the more important political offices, (2) the various parties and other organizations of the capitalist class which exercise decisive influence over who holds state office, their decisions and the conditions affecting these decisions, and (3) the structural constraints of the capitalist system as such which provide the real options and preferred goals of any state action, given that the system itself is never put into question. Despite criticisms of his earlier work by Poulantzas and others, however, Miliband continues to underplay (not ignore) the last of these elements. Thus, though Miliband mentions various economic functions of the capitalist state, they are never integrated into the organic processes of the capitalist mode of production. What the processes of capital accumulation and the production of value require of the state, any capitalist state, if it is to succeed in reproducing the conditions of capitalist existence, is never explained. Granted that the works in which these requirements are laid out in greatest detail tend to offer an overly deterministic account of political activity, understating them is equally destructive of Marx's meaning. For example, one never gets from Miliband, as one does from Marx and his more dialectical followers, a sense of the workings of the whole capitalist system from the vantage point of whatever aspect or sector is being examined.

Another aspect of Marx's broad theory of politics—his view of the state as the alienated social power/illusory community, which might have served Miliband as an alternative mode of viewing capitalism within the state as well as the state within capitalism—is ignored altogether (hence, though alienated politics is often described, it is never theorized). The result is a study of politics as a sector of capitalist life (as opposed to an expression of capitalist existence), an extremely competent study as far as it goes, but partial, one-sided and necessarily distorting.

Miliband's attempt early in the book to define the proletariat is one such distortion. Only after the system in which classes operate has been delineated, at least in broad outline, can one hope to say who falls into which class. This is true not only because it is the whole which gives meaning to the part but also because the meaning of the part alters somewhat as a function of its changing relation to the whole (a relation that changes with the development of the whole *and* with the different perspectives—themselves related to different purposes—that are adopted for viewing it). Hence the elasticity of the boundaries Marx seems to draw around different classes, and the fact that he sometimes places the same group in one class and sometimes in another. When, after distributing the population among various classes, Miliband then goes on to raise the question of class consciousness, he can only offer a one-sided, psychological response. The real social processes of capitalism that are constantly remaking the actual, probable and possible dimensions of our consciousness are simply not available at this point to be introduced into the discussion.

Finally, Miliband's sectoral analysis of the capitalist state adversely affects his understanding of the relative autonomy of the state, currently the subject of much debate among Marxists. For Marx, the state can be relatively autonomous (1) in relation to actual members of the capitalist class (the case when a majority of high political offices are held by members of other classes), (2) in relation to the influence of the organizations of the capitalist class (the brief periods when this influence is not decisive), and (3) in relation to the structural constraints of the capitalist system (the occasions when the rules of the game can be broken or at least bent). Relative autonomy in this third sense generally indicates a transition period between modes of production. Underplaying the organic connection between the state and the capitalist mode of production, Miliband concentrates on relative autonomy in the first two senses; he misses the ways in which a state that is relatively autonomous in these senses—conditions met by both social democracy and fascism—is still tied to its ruling class, is still a class state.

In sum, the state's ties with the capitalist class are ties which permeate the whole capitalist system, as society, economy and culture, and as illusory community. By restricting his study of the state to one sector of capitalist life, Miliband necessarily misconstrues the state both in its identity with capitalism (as a part which gives expression to the workings of the whole) and in its relative autonomy within capitalism (as a part with a distinct though changing role in the operation of the whole.

Still, all things considered, I have no hesitation in recommending *Marxism and Politics* as an extremely clear and useful introduction to this subject (maybe the best available), although—for reasons given—a fully adequate work on Marxism and politics remains to be written.

BERTELL OLLMAN

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Anarchism: Nomos XIX. By J. Roland Pennock and John W. Chapman, eds. (New York: New York University Press, 1978. Pp. xxxix + 375. $17.50.)

Anarchism is a world of thought, feeling, and action not easily captured in any book. Its sides and strains are legendarily diverse. *Anarchism* edited by Roland Pennock and John Chapman is the latest in their justly admired *Nomos* series and it is quite successful in several ways. It has a number of thoughtful essays seeking definition of anarchism, no undemanding task. Especially notable here is John Clark's opening discussion. It has able arguments about anarchism which do not tread completely familiar ground. David Wieck's challenging defense of social anarchism is one example. Alan Wertheimer, as always, presents a strong essay (if not always convincing in its distinctions) which becomes a forceful attack on anarchism.

Perhaps the greatest single strength of the book lies in the attention given to the role of authority in anarchism, a subject which leads its discussants on to a number of other crucial dimensions of anarchism. Richard DeGeorge's essay seems rightly to have stimulated other authors, including Richard Wasserstrom who denies DeGeorge's claim that anarchism is inextricably involved in authority, and Rex Martin who supports DeGeorge on different grounds. Fortunately, Alan Ritter contributes to this debate also, despite his puzzling belief that anarchism is mostly about rational deliberation (based on a too ready acceptance of Godwin as the paradigmatic anarchist). To this same debate, ably carried on by DeGeorge, Ritter and the rest, belong good papers by Donald McIntosh, illuminating the anarchist taste for authority and Grenville Wall, advocating the rediscovery of authority. Altogether it is a lively debate and a highlight of this volume.

The general conclusion of this discussion is that anarchism by no means may be equated with the total rejection of authority. The crucial, distinguishing features of anarchism turn on the legitimate kinds, sources, and limits of authority for anarchists. This understanding of anarchism makes it a far more sophisitcated vision than it often seems in the hands of some of its opponents—and proponents. Little routine stereotyping is present in this book. Anarchism gets taken seriously, though too few participants are in sympathy with anarchism for the book to have as much balance as it might.

One of the controversial elements in this book's approach to anarchism is as predictable as it is problematic. The tendency here is to treat anarchism in the "cautious, sober, analytic style" (p. 168) which is natural for academics but not self-evidently reflective of the anarchistic mood or message in any age. Anarchism has often been about passion, feeling, energy, Some of the authors in *Anarchism* recognize the limits of a sober, reasoned emphasis, though many do not. Lester Mazor speaks in the language characteristic of anarchists; April Carter probes the attractions of violence for anarchists; and Alan Wertheimer appreciates that anarchism has often chosen less than "rational" words. But often this side of anarchism is missed here.

Moreover, *Anarchism* is most interested in anarchists who wear the dress not only of sober reason but especially of philosophical reason. This explains the considerable attention devoted to Robert Paul Wolff. Several articles, including Patrick Riley's devastating critique of Wolff's use of Kant "in defense of anarchism," deals with Wolff's well-known position. In this work the impression that Wolff is the most effective philosophical voice for anarchism reigns supreme. Yet one wonders if Wolff is that important in this context. Is *In Defense of Anarchism* a seminal book in the history of anarchism? To be sure, anarchism's philosophical library is small and Wolff's short essay renewed a number of important issues. Yet I wonder if either Wolff or politically active anarchists would rank Wolff's essay so highly. The communitarian anarchists might rather pay attention to Murray Bookchin and the libertarians might turn to Murray Rothbard. Wolff is their superior as a philosopher. But is anarchism about philosophy? Can one learn best about it through its philosophical side?

Anarchism shows little interest in the so-called greats of anarchism (though there are exceptions, especially Alan Ritter's article). This emphasis may make sense, given the editors' philosophical orientation, though a case could be made that for many anarchists anarchism *is* Kropotkin, Bakunin or Thoreau, philosophers of note or not. Similarly, there is almost no attention given to any of the classic anarchist experiments (failures?) of the past. Again, that anarchism might be mostly a historical ideal or myth or passion is not this book's conception.

On the other hand, there are a number of intriguing essays which focus on practical issues. Philosophical anarchism is not the only subject in *Anarchism.* These usually concern the viability of contemporary libertarianism and are very much worth pondering. The current fashion of libertarianism receives a great deal of attention in essays by Eric Mack, Murray Rothbard, James Buchanan, and Christopher Stone. Often their treatment is fascinating and certainly it is topical in this age of criticism of the state. The larger questions are whether libertarianism is anarchism and, if it is, if this form merits as much discussion as it receives here. Surely it does in a contemporary American context. It probably does not, if one looks an anarchism in a historical or international perspective.

Such conceptual questions aside, Pennock and Chapman have produced another work of mature reflection by thoughtful scholars. It has much to teach anyone interested in anarchism. It concludes with an excellent bibliography by Robert Kocis containing almost every major work on anarchism (though W.O. Reichert's *Patterns of Freedom* is unfortunately missing).

ROBERT BOOTH FOWLER

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Max Weber Bibliographie: Eine Dokumentation der Sekundaerliteratur. By Constans Seyfarth and Gert Schmidt. (Stuttgart: Ferdinand Enke Verlag, 1977. Pp. xxvii + 208. DM 28.)

Max Weber is the overpowering figure in the post-World War II social sciences, more often quoted and referred to than Marx and Freud, and less often the object of partisan acceptance or opposition. Whether as a sociologist, political scientist, social and economic historian—I would say he is a historical sociologist—Weber's imprint is unmistakable. Consequently, there is no end of Max Weber literature, significant literature and me-too literature, making it nearly impossible for the searching scholar to know what's what. In this context, the *Max Weber Bibliographie* of Constant Seyfarth and Gert Schmidt is more than welcome, it is indispensable. We must be grateful for having it.

The bibliography is a testimony to the now-defunct Max Weber Institut at the University of Munich. It is based on a collection of titles by Paul Honingsheim and on a complementary collection by Johannes Winckelmann, gathered at the Munich Institute. The collection has been enlarged by the editors, with the help of many collaborators, to reach the present size of over 2,300 titles. The bibliography has three parts: section A contains the editions of Weber's writings since 1920 and the translations. Section B contains books, collected essays, special editions of journals and unpublished dissertations essentially dealing with Weber. Section C contains articles, chapters from books and larger essays from newspapers. Section C is chronologically ordered, but there is no structuring of the literature according to the subject areas of Weber's work. Indeed, any structuring of this kind would be exceedingly difficult because of the need for cross-referencing which would arise. There is, however, apart from an author's index and an index of publications in various languages, also a subject index; an evaluation of this latter index shows how complex a task it is to get every contribution into the proper box. The editors have done the best they could.

More important than technicalities—one can always improve—is the question as to what the scholarly community will do with a comprehensive, or nearly comprehensive Weber bibliography. The editors draw attention to the fact that there is a "contextualized" and a "decontextualized" aspect to the reception of Weber thus far. By "contextualization" is meant an orientation toward a better understanding of the historicity of Weber's work, by "decontextualization" an orientation toward an improvement of Weber's concepts and theoretical propositions. As a matter of fact, both orientations go hand in hand. One cannot understand, for instance, Weber's emphasis on a "value-free" scholarly endeavor without taking into account his aversion to the emotional value-emphasis in the garb of the science of academicians like Treitschke and Schmoller or his two-pronged predilection—a value emphasis on Weber's side—for the type of the hero or the saint against the type of the plebeian or the bourgeois without being aware of the strong influence of both Nietzsche and Tolstoi over Weber's mind. No matter which aspect one emphasizes, the significance of the attraction of contemporary scholars to Weber is that social scientists increasingly feel that they should turn away from positivistic snapshot sociology to a historical approach, again, following the editors, in a twofold way: we need to return to classical formulations of social and political thought and in doing so we need to combine the descriptive analysis of the historian with the conceptual ability of the social scientist. The outcome, still insufficiently grasped, is ideal-typical methodology, which has its own pitfalls, but which enables us to pay attention both to the uniqueness of each case and to its comprehension in the context of scientific categories. Not Weber as an expert on religion, on domination, or on bureaucracy ought to emerge from a proper use of the Seyfarth-Schmidt bibliography, but Weber as a teacher of methodology.

WERNER J. CAHNMAN

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Nature and Civilization: Some Implications for Politics. Mulford Q. Sibley. (Itasca, Ill.: F. E. Peacock Publishers, 1977. Pp. xiii + 319. $10.50.)

Nature and Civilization follows Henry S. Kariel's *Open Systems* as the second series in the "Loyola [of Chicago] Lectures in Political Analysis"—a series that permits contemporary political philosophers to address themselves to modern political questions for a normative perspective" (p. xi).

The avowed purpose of Mulford Sibley's rather ambitious work is to "help establish a perspective on the never-ending puzzle of man's relation to nature and civilization" (p. xiii)—a perspective which sketches his vision of modern political problems against the backdrop of holistic and teleological political philosophy extending back to Plato and the early Hebrew teachers. He hopes that this vision will initiate a dialogue between him and his readers—in this hope I think he succeeds.

Initially, Sibley argues that his reflections on nature and civilization attempt to resurrect a holistic, teleological approach to political thinking focused on the central question of what humanity can and should become. Politics within this tradition represents the conscious deliberate effort to control, direct, and order collective life.

After defining his categories of nature, human nature, and civilization, Sibley addresses the modern political challenges—(a) authority, coercion, and freedom; (b) technology, (c) the politics of the economy, and (d) the problem of utopia building—within the framework of these categories. Although Sibley is eclectic and draws upon components of many political thinkers, he relies heavily upon classical thought and with the Greeks he believes that there is a *human* nature which transcends cultures and this nature is composite. This human nature—both beastly and divine—is "the root of many of our psychological, ethical, and political problems" (p. 13). This emphasis upon the recognition of the tension of human existence is healthy for a discipline that has tilted too far to the empirical-material end of the "nature-human nature" spectrum.

Next, Sibley anlyzes justice—the central issue of political philosophy—and the modern *problem of authority, freedom, and coercion.* With specific reference to justice, Sibley argues that the issue

> is to discover primary "ought" propositions, from which we can, given logic combined with empirical investigation, derive specific advice on what we should do about our personal lives and communities.
>
> These "ought" propositions at the primary level are revealed or intuited or self-evident. . . . [T]he process of intuition on discovering starting points for discussion of justice is not, or ought not to be, a solitary one: We exchange experiences of primary values and value hierarchies with our fellow human beings. . . (p. 66).

This question of starting points, I believe, is the central question for political philosophy, for it is upon the answer to this question that we build our vision of justice in communities. It is also central for Sibley's arguments because his final chapter—"Nature, Civilization, and the Problem of Utopia"—is the culmination of his philosophical reflections. He argues that "the student of politics has an obligation not only to explain and criticize but also to propose and explicate ideals" (pp. 47–48). Utopia building is imminently practical for it "helps save us from the illusions which both revolutionary leaders and defenders of the status quo constantly tend to propagate" (p. 254) by providing a long-range goal that prevents the acceptance of short-run panaceas and a vision of what humanity can and should become.

Moreover, utopia building is a uniquely human activity because it relies upon the power of imagination—it is an attempt to "imagine a more nearly ideal society" (p. 47). The imagination becomes the human capacity which enables us to find "starting points." Finally, utopia building evidences the political dimension of human nature, because it represents the attempt to deliberately direct and order community affairs.

In conclusion, I ask several questions addressed to the issue of "starting points"—questions I would have liked to have seen Sibley grapple with in more detail. Can the "starting points be rationally authenticated? Does rationality—the logic of means-ends calculation—apply to political thinking only after intuition or revelation or imagination *discover* the "starting points?" Must "starting points" always depend upon social confirmation? Can a particular vision be correct without social articulation? How do we judge?

What is imagination? Intuition? Revelation? Are they manifestations of the same human capacity? Are they discrete capacities? Can these faculties perceive (or discover) a reality? Or do these faculties create an ideal reality?

Finally, is Sibley's visionary utopia—described in the present tense in the final chapter—rooted in his discovery through imagination of a reality which his utopia accurately translates? One hopes that the dialogue begins here.

Mulford Sibley has written a provocative book.

CHARLES R. EMBRY

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The Politics of Social Knowledge. By Larry D. Spence. (University Park: Pennsylvania State University Press, 1978. Pp. vii + 374. $15.00.)

This is the kind of book which inhibits a reviewer's propensity to represent disagreements as criticisms. Each page makes one wish to stop and interrogate Larry Spence and press him to elaborate the defense of his claims and conclusions, but an important argument is being enthusiastically and intelligently developed. I am neither entirely persuaded that the argument is adequately formulated nor convinced by the evidence adduced, but Spence makes it clear that his enterprise is basically one of "conceptual uprooting" and that "there is no attempt to prove or demonstrate the argument" (pp. 3, v). He wishes to conduct "a critical examination of some problematic assumptions of Western political thought" and to present "a plausible alternative that should be researched, criticized, and tested" (p. v). He succeeds in this endeavor. The assumptions which he examines are that human nature is inherently deficient and that, as a consequence, society must inevitably be hierarchical. The alternative which he proposes is the idea that a classless society is feasible and desirable.

Spence argues that claims to knowledge by "heroic" theorists and investigators, from Plato to modern social scientists, usually involve an assumption of special access on their part and an endemic ignorance in most, or all, of the rest of society. Such claims ultimately constitute a denial of the possibility of social knowledge and create a demand for hierarchy and social education based on myths and dogmas that serve the theorist's vision of truth. This intimate but paradoxical connection between claims to social knowledge and the perpetuation of social ignorance and hierarchy is lucidly and forcefully presented, but equally significant is Spence's original and stimulating critique of the theory and practice of socialization research and its assumptions about explaining and controlling knowledge and behavior. He argues that the premises of various schools of socialization theory are fallacious, internally inconsistent, and unsupported or contradicted by empirical findings. Social science should, he suggests, turn its attention toward the understanding and control of organizational structures rather than the manipulation or reform of social action. Spence maintains that hierarchical organizations produce functional cognitive damage to the human organism. They injure the capacity for concrete activity among the elite while restricting the ability of those on lower levels to engage in abstract thought. The result is a pathology of productive capability. Learning and the discovery of knowledge become not only difficult but also harmful in the context of adapting to organizational demands.

Spence argues that "knowing is something we do instead of something that happens to us" (p. 160) and that it is grounded in the verbal capacities which underlie theory and theorizing. Theories or cognitive maps provide a basis for systematic cooperative investigation of the world and for the improvement of the quality of human life. Traditional political thought, however, with its emphasis on reordering a chaotic world, as well as recent analyses of theory in the philosophy of science such as that of Thomas Kuhn, imply a need for dogmatic restraints on creative theoretical activity and a need for "cognitive hegemony" and the "hierarchical social organization" that follows from it. What is required, Spence argues, are social contexts that encourage experimentation, and this in turn requires a view of human beings that rejects the notion that they are necessarily defective creatures who must remain bound in varying degrees of ignorance. Drawing heavily on the arguments of the social biologist Trigant Burrow, Spence suggests that the repression of basic, organically grounded social instincts by symbolic processes in certain conventional situations results in social neuroses which restrict the development and free play of communicative skills. Since hierarchical organizations limit learning and knowing, it is necessary to create social and political institutions that foster human autonomy and development. Hierarchy leads to a schizophrenic condition such as that described in the "double-bind" theory of Gregory Bateson and the work of R. D. Laing, and the result is an atrophication of communication and theory. The goal of social science should be to undertake a critical examination of institutions that create such pathological conditions and to experiment with new structures.

My only criticism of this book revolves around Spence's use and treatment of the literature that he discusses. The material he introduces in defense of his thesis is often not scrutinized. For example, the controversial cognitive-development theory of Laurence Kohlberg is presented with only the mild caveat that "we cannot, of course, take it that he has proven his case once and for all" (p. 120). On the other hand, the arguments of individuals whom Spence interprets as being in conflict with his own position are sometimes conveyed by summary and prejudicial characterizations which reflect the comments of their critics. Although Kuhn's work is discussed at some length, he is repeatedly described in such terms as presenting "prescriptions" which are "destructive of the knowing ability" (p. 9) and as suggesting that it is possible to "obtain brilliant progress in the sciences by means of a commitment to mediocrity and orthodoxy" (p. 200). But more important: the relationship is not entirely clear between Spence's own argument and the diverse ideas from various fields that he refers to in the course of developing that argument. There is also a failure to discriminate sufficiently between works of very different quality and significance, and, at times, the attempt to deal with so much literature is distracting.

Despite this difficulty, the book itself exemplifies the active and critical exercise of mind that Spence is advocating. Whereas so much of what passes as political theory involves ever more precious interpretations of the classic texts and spurious allusions to contemporary politics, inflated pronouncements about the meaning of the great tradition, pretentious and irrelevant academic moralism, summaries and paraphrases of arguments from various branches of philosophy, the elaboration of arbitrary abstract conceptual frameworks, and strained generalizations from aggregate data, Spence's sustained attempt to make a substantive argument about the nature of political phenomena and how political institutions should be constructed constitutes a genuine and creative act of theory.

JOHN G. GUNNELL

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Anamnesis. By Eric Voegelin. Translated and edited by Gerhart Niemeyer. (Notre Dame, Ind.: University of Notre Dame Press, 1978. Pp. xxii + 217. $11.95.)

This volume first appeared in 1966 in German under the same title. Those looking for the "theory" behind Voegelin's new science of politics will eagerly turn to this work for a concise yet authoritative exposition of his most important thoughts. "Anamnesis," the Greek word for "recollection," is also the title of the first of the book's three parts. This title was undoubtedly suggested by the Platonic doctrine that "learning is recollection." It is apt for two reasons: first, because both the Platonic doctrine and Voegelin's book are ontological theories of consciousness, and second, because Voegelin's argument for developing such a theory rests upon his recollection of the political and ideological disorders of interwar Europe and the inability of the academic establishment to understand these disorders. No rational argument could persuade the Fascists or Communists to abandon their positions because part of those positions consisted of a "resistance to truth." What was needed, therefore, in order to understand the times, was an investigation of the resisting consciousness which would also require an investigation of consciousness open to truth, i.e., an analysis of consciousness as "the specifically human mode of participation in reality" and an understanding of history as "the permanent presence of the process of reality in which man participates with his conscious existence." Part 2, "Experience and History," begins the development of such a theory while the political significance of such a theory is revealed in part 3 where Voegelin explores the question, "What is Political Reality?" or as it might alternatively be stated, "what is political participation in reality?" political reality being the subject matter of political science.

Key to Voegelin's enterprise is the concept *nous* which he develops under the guidance of Aristotle. *Nous* is the specifically human participation in the order of being. "At its core, human nature . . . is the openness of the questioning knowledge and the knowing question about the ground [of being]. Through this openness, beyond all contents, images and models, order flows from the ground of being into man's being. . . . Questioning knowledge and knowing questioning . . . [are called] noetic for it is not the experience of some thing, but the experience of questioning arising from the knowledge that man's being has not ground in itself" (p. 85). Practical wisdom (*phronesis*), therefore, is achieved by the man who lives in the tension between the eternal ground of being and the mutability of human things and who tries to give concrete expression to what is right by nature; it is the "locus at which the movement of being in man becomes reality. . ." (p. 65). Therefore, political science, or the noetic interpretation of the order of society, as Voegelin prefers to call it, cannot consist of a corpus of propositions because its object, political reality, is itself conditioned by another type of knowledge, the society's interpretation of itself in the light of its non-noetic experience of being. Political science exists in the tension between itself and the ground of being and between itself and society's non-noetic self-interpretation, and its derailment into ideology consists of an ill-conceived objectivization of knowledge, consciousness' drawing of the reality of the ground into itself. The severing of the nexus between consciousness and its ground makes possible the substitution of contrived realities for the one which is given both in thought, which is the essence of ideological thinking, and in political action, which is the essence of ideological activism.

THOMAS F. PAYNE

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About Possession: The Self as Private Property. By John R. Wikse. (University Park and London: Pennsylvania State University Press, 1977. Pp. ix + 169. $12.95.)

In *Philosophy of Right,* Hegel presented the institution of private property as an embodiment of human personality. In *About Possession,* John Wikse meditates on the idea that the distinctively modern form of personality, "this self with which I am alone is conceived on the logic of private property" (p. 1). It is an intriguing idea, and Wikse dwells on it through many appearances. His book is studded with flashes of insight that should provoke readers to meditate further themselves.

The thrust of Wikse's exploration is that "modern idiocy" (from *idiotes,* a private and separate person) and its logic of behavior lead to despair. If my "self" is property, then I must keep others from having it; "unless I hold onto my authentic self, wrenching it away from the bog of social conformity, unless I possess myself truly, then I am at-a-loss" (p. 17). But this is an impossible effort, leading to dilemmas and despair, for the conception of the self unavoidably involves "a deep structure of political assumptions which we share with others despite what we might think of ourselves" (p. 9). In this perspective, alienation and authenticity turn out to be alternate sides of the same counterfeit coin.

Wikse offers an "autobiographical account of my own idiocy" (p. 9), bravely risking stereotyped and defensive reactions. But there is value in his sharing his effort to understand how he was taught that "of course it hurts to be a man" (p. 10). I wish he had devoted more time to "the discipline" and how it hurts to be a political scientist. He uses etymology to illustrate and hammer home (but not, I must add, to provide reasons for believing) that the same logic pervades modern culture's psychopathology and its political philosophy. "The law of personalty is to personality as the law of realty is to reality" (p. 37).

The body of the work is devoted to showing how Wikse's subject shows up in a variety of thinkers: Luther, Hobbes, Rousseau; Hegel, Kierkegaard, Heidegegger, Sartre; Adams, Madison, Emerson; Nietzsche. Wikse's effort is to articulate assumptions. "Many of the problems which characterize the tradition of modern Western political philosophy—the relationship between self and society and the tension between power and freedom particularly—are logical extensions of the dilemmas of the desperate debtor. . . . These problems are intelligible in that they reflect the conundrum of a tradition which has continuity in the degree to which it does not see through its characteristic assumptions" (pp. 89–90).

Wikse's work is thus a fresh angle of attack on certain premises of liberal individualism, in which immanent critique leads to the discovery of contradiction, dilemmas, or despair. There is something very right-headed about it, and it shows an engagingly candid and powerful mind. But the book has flaws. It characteristically confronts the theorists treated on Wikse's own ground, not theirs, so that the plausibility of its interpretations is usually challengeable. Given his thesis, Wikse should have paid far more attention to Locke, Hegel, and Freud. "Private property" is treated as a single, simple, unproblematic idea; the book offers no analysis of "property." Although Wikse occasionally claims Marxist leanings, in the main the stance of the work as political theory is quite conservative: "It is not an argument" (p. vii). "I have sought no way out, only perspective" (p. 6). The bird in Wikse's hand is a species of Minerva's owl, but it offers no reconciliation.

JOHN R. CHAMPLIN

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American Politics

Politics and the Professors: The Great Society in Perspective. By Henry J. Aaron. (Washington: The Brookings Institution, 1978. Pp. 185. $9.95, cloth; $3.95, paper.)

The optimistic faith in positive government and the broad liberal consensus that were born of the Great Depression, World War II and the postwar reconstruction of Europe culminated in the 1960s with the enactment of the War on Poverty and the Great Society legislation. Fifteen years later, there is widespread disappointment that those vast social policy undertakings have not solved the problems of poverty, racial discrimination, and unequal educational opportunity. Most of the programs are still with us, but doubt about the ability of government to accomplish positive social change, certainly that government is unnecessarily intrusive and horribly inefficient have replaced the earlier liberal consensus.

Henry J. Aaron, formerly a senior fellow in economics at the Brookings Institution and currently Assistant Secretary of Health, Education, and Welfare for Planning and Evaluation, explains the decline of the consensus and analyzes the evolution of attitudes of scholars and popular writers concerning the ability of the federal government to bring about beneficial change in the broad areas of poverty and discrimination, education and training, and unemployment and inflation. His book is not a defense of the Great Society or a plea for its return to center stage among national priorities. His thesis is that the political demise of the Great Society was not primarily due to the failures of its programs, but occurred because of three external factors: (1) the Vietnam War and Watergate which led to a loss of faith in governmental action and integrity; (2) the dissolution of its supportive coalition following the successful conclusion of the legal and political phases of the civil rights movement; and (3) the collapse of the intellectual consensus that underlay the goals of the War on Poverty and the Great Society.

Aaron demonstrates how social science research contributed to this third factor. Social scientists generated conflicting interpretations of the nature of poverty (cultural versus environmental) and contrary solutions to the problem of unemployment. Scholars such as Coleman and Jencks produced studies that indicated that improved education and training could do little to end unequal academic and economic performance by individuals. Most recently, economists have been unable to provide much guidance on how to reduce unemployment and limit inflation simultaneously. Aaron does not believe that social science is capable of providing policy makers or the public with answers to such complex problems. Quite the contrary, he has a very modest view of the appropriate role of social science in the policy process. Most of the time, he points out, research findings follow rather than precede policy changes. Two recurrent themes in the book are that research reflects prevailing popular moods at least as much as it shapes them and over time research tends to have "profoundly conservative" effects (p. 17). It is essential that scholars and policy makers recognize these tendencies.

The major contribution that the social sciences can make to public policy, Aaron concludes, is to provide "a perspective, and encouragement to evaluate programs in terms of their demonstrable effects" (p. 165). To do this, analysts must work to "raise the standards of admissible evidence" (p. 166) and to improve their analytical techniques. He emphasizes, however, that research findings are only a small portion of the total evidence on which policy decisions rest. Ultimately, policy depends on underlying values which are properly subject to debate but which, by their nature, cannot be the subject of research.

No doubt Aaron's limited assessment of the political impact of social science research will draw sharp criticism but it seems to me sound and carefully argued. The limited explanatory and predictive powers of the social sciences, including economics, are recognized by most of its objective practitioners. (Those who are not so objective would do well to take stock of their capabilities). All of us should recall that the centrality of values, faiths, and beliefs in the processes of political choice has been understood since the time of Plato and Aristotle. This informative and insightful book makes us aware that modern social science can help to make those choices more rational but it cannot expect to do more.

NORMAN C. THOMAS

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Political Corruption in America. By George C. S. Benson, with Steven A. Maaranen and Alan Heslop. (Lexington, Mass.: Lexington Books, D.C. Heath, 1978. Pp. xviii + 339. $21.95.)

Woodrow Wilson once spelled out a plan for reform by saying, "I am going to teach the South American republics to elect good men." Sixty years later, Benson, Maaranen and Heslop tell us that "the election or appointment of better officials is the one relatively secure method of eliminating corruption" (p. 15). Corruption, they feel, is a product of low moral standards and inadequate "basic ethical education."

This book is a general inquiry into corruption in America. An opening chapter on techniques of corruption is followed by seven chapters reviewing its history. Then come chapters on elections, the costs of corruption, a review of theories of corruption, and finally four chapters on contemporary political ethics. A brief introduction promises "unusual theses" and "a thoughtful, chronological history of corruption in America" (p. xvii).

Unfortunately, the book does not live up to these promises. The seven chapters of history—repetitious and often vague on evidence and citations—are numbing. The authors then jump to proposed remedies, without taking the necessary step of analyzing why corruption develops and persists, except to attribute it to weak moral standards. Thus we find, on p. 124: "The basic reason [for corruption in Maryland] is perhaps the bad ethical habits which Maryland legislators have acquired."

The problem is that corruption, paradoxically, is most difficult to analyze in moralistic terms. To attribute it to "bad ethical habits" is not far removed from saying that people are corrupt because they are corrupt. Corruption is a form of influence; it must be analyzed within the context of whole polities, and their systems of rewards and sanctions, just as we would study any other form of influence. The authors confuse a legitimate *reaction* to corruption with *explanation,* and are left to grapple with the age-old question of why people break laws. (I suspect that question *is* age-old because it has no clear and satisfying answers.) Moralistic approaches can be helpful:

the authors could have applied concepts of political culture, the work of Kohlberg, or the sociology of deviance, but they did not. The tone is instead atheoretical, the evidence anecdotal and impressionistic. Why not analyze systemic causes of corruption? Routine decision making, even when administered by the most moral individuals, constitutes a bottleneck between what people want from government and what they get, creating a built-in temptation to circumvent the rules. Yet we find nothing about this; indeed, the authors term non-moralistic analysts "social science apologists for corruption" (pp. 187, 205).

Because they have no theory of why corruption develops and persists, Benson et al. can only suggest the most general of remedies. Administrators, they argue, should give talks and send out twice-yearly memos emphasizing honesty (pp. 160, 247). For others, they suggest "ethical instruction"—this despite admitting that such instruction, which they claim was much more prevalent in nineteenth-century America, did little to halt the corruption of that era (p. 278). Why the authors—after devoting seven chapters to showing that corruption pervades all levels of government, is tied to organized crime, and reaps millions of dollars of profit for its perpetrators—feel that memoranda and moral suasion will clean up today's politics is unclear. Their interesting proposal that federal block grants to law enforcement agencies bear strict, comprehensive auditing requirements is given only scant discussion.

Other problems arise: after discounting structural causes of corruption, the authors claim that *all* council-manager cities "have made themselves substantially more honest" (p. 114), apparently just by adopting the council-manager plan. And there is the odd treatment of Watergate: it is not directly discussed for reasons of brevity, and because it is "out of the mainstream of American corruption" (p. xvi). Occasionally, however, the authors do raise Watergate, contending that "President Nixon was driven out of office by the media for assenting to the coverup of two third-rate burglaries" (p. 242) and for "covering up minor offenses" (p. 294). This is a narrow conception of Watergate, and an odd stance indeed in a book which otherwise argues that wrongdoing is wrongdoing and should always be punished. Shouldn't moralists regard Watergate, and the threat it posed to civil liberties and the accountability of executive power, as a most serious kind of corruption? In any event, if the authors have an argument to make about Watergate, it should be made explicit.

Benson, Maaranen and Heslop offer worthwhile observations on several topics, in cluding corruption at the state level, and links between organized crime and corruption. But these contributions must be weighed against two serious shortcomings: first, the book does not examine the "why" of corruption, other than to blame it on low moral standards; and second, it leads the reader toward vague, atheoretical suggestions for reform, without presenting credible evidence of even a single case in which "moral education" has stopped corruption. Corruption is a complex issue, and deserves a more systematic treatment.

One final point: the entire volume suffers from poor editing and typography. Especially disconcerting are inconsistent or missing citations, and the many dates which are simply wrong. Where the blame for this lies is unclear, but for $21.95 we are at least entitled to clean copy.

MICHAEL JOHNSTON

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Small Cities in Transition: The Dynamics of Growth and Decline. Edited by Herrington J. Bryce. (Cambridge: Ballinger Publishing Co., 1977. Pp. xxv + 418. $22.40.)

Since the last census, there seems to have been a reversal of the historic trend towards increased concentration of the American population into its largest cities. The largest central cities have experienced absolute reductions in population, while middle-sized cities and smaller metropolitan areas have grown at an accelerated rate. American social science has imitated this trend by showing more interest in suburbs and small cities, as evidenced by this collection of 16 papers on the subject, given at a 1976 conference sponsored by the Joint Center for Political Studies.

Although most of the articles are overwhelmingly empirical, two of the better articles produce no data of their own, but step back to review the significance of the recent demographic trends. Werner Hirsch forecasts a new stage in urban settlement patterns which he calls "the polynucleated metropolis"; partly in response to increasing transportation costs, this stage will feature numerous medium-sized population centers within a single metropolis. Wilbur Thompson, however, makes a caveat; net out-migration seems to be more closely associated with old-line manufacturing cities than with large cities *per se*. The current movement out of manufacturing cities into Sunbelt and smaller cities may simply build a new set of large cities, in spite of our growing preference for small ones.

The recent shifts in population have produced a significant proportion of declining cities as well as several boom towns. The contrast between growing and declining cities is the subject of two articles by the editor and Thomas Muller. These show that in the long run population decline is linked to economic decline by way of rising vacancy rates, accelerated blight, lower retail sales, and loss of private sector jobs. The result is that the city government itself becomes ever more important as a source of jobs for its citizens; this may help explain the observation that declining cities continue to increase the number of employees on their payrolls. Annexation, on the other hand, seems to be closely related to demographic and economic well-being. Cities that are in a position to do so try to maintain their viability by a competitive game of territorial expansion, while cities with no room for expansion on their borders are more likely to begin to decline.

Two articles focus specifically on small and suburban cities with large black populations. Harold Rose identifies the historic black enclaves, as well as those older cities with less expensive housing which have been the more recent centers of black immigration. The recent suburban black movement should not be mistaken for suburban integration; even in Los Angeles, the metropolitan area with the most suburban blacks, most blacks are still concentrated in majority-black neighborhoods in a few jurisdictions. Phillip Clay points to clear indications of a "psychology of disinvestment" and a potential "suburban crisis" in these cities (p. 312).

The articles of most general interest to political scientists are those on federal and state policy towards urban areas. A common theme of these articles is that, for political reasons, federal aid is incorrectly targeted. Stephen Barro points out that the trend toward formula grants result in an increased dilution of tax dollars; John Shanonon and John Rose show that revenue sharing is even more diluted, because its formula guarantees that even the wealthiest cities get some of the money. As a result of the congressional "iron rule" that everybody must get something, there is almost no correlation between municipal hardship and municipal dependence on general revenue-sharing funds. Barro also examines the possibility that federal policy may be even more directly responsible for the urban crisis because of the ignored long-run locational impacts of federal highway and transportation policy, tax policy, and labor policy, which have shifted economic activity to the suburbs and to the Sunbelt.

While the book is quite valuable for some purposes, it suffers from some of the nearly unavoidable problems of convention proceedings—including some wordiness. A more serious limitation, in my opinion, is the tendency in a majority of the papers to simply present the data in endless tables with only the most obvious of interpretations. The level of data analysis is by and large rather simplistic, only rarely rising beyond percentages and regional means. The only modest attempt to develop a model and test hypotheses appears in the editor's final chapter, which finds that regional economic diversification increases with population. Although the book would have been much improved with more attempts at model-building of this sort, it remains a valuable resource book for the data presented and the insights provided into the major current demographic trend in America.

GARY J. MILLER

California Institute of Technology

When Farmers Voted Red, The Gospel of Socialism in the Oklahoma Countryside, 1910–1924. By Garin Burbank. (Westport, Conn.: Greenwood Press, 1976. Pp. xvi + 224. $13.95, cloth.)

Garin Burbank has made a significant contribution to the history of American political behavior and the study of social movements. His analysis is a creative combination of qualitative and quantitative data bearing on the question of who the Oklahoma socialists were and what they thought.

The socialist movement in Oklahoma in the years between the granting of statehood and America's entry into the First World War was a significant political force which threatened to become the "second" party in a state dominated by Democrats. The Oklahoma branch of the Socialist party of America was in fact the largest state party in the nation with 3,025 members and 342 functioning locals in 1913. Its social base of support is apparent in any examination of the relevant census and electoral statistics of the period. Burbank writes: "The . . . Socialist vote is readily identifiable; correlations can be made with low farm values, high rates of farm tenancy, greater percentages of rural population, slower rates of population growth, greater percentages of mortgaged property, and dependency upon wheat and cotton as staple crops" (pp. 5–6). Except for the isolated coal towns of the southeastern counties, Oklahoma socialism was a movement of the rural masses, pitting the debtor class of small landowners and tenants against the town-dwelling middle classes of bankers, landlords, lawyers, and merchants.

The translation of the farmers' real economic hardship into support for the Socialist party was made possible, according to Burbank, by the local party's deviations from national party orthodoxy, deviations which facilitated the party's success in attracting support in a rural setting. While the national party believed in the inevitability and desirability of large-scale, collectivized agriculture, local socialists, realizing the national platform would never sell with the highly individualistic farmers, promised instead that socialism would make the family farmers more secure in the possession of their homesteads and provide the landless tenants with their own plots from the public lands. While socialism was ideologically hostile to religion, Oklahoma socialists tended to fuse the doctrine of class struggle with fundamentalist Protestant theology. Transformed socialism appeared to be the movement of the hard-working and morally righteous in struggle for the Christian community. While the national movement advocated the unity of all the workers regardless of race, the local party confronted the entrenched doctrines of white supremacy and racial separation. Local activists could not afford to offend unduly the racial sensibilities of the white farmers upon whose support the party's electoral strategy depended (blacks were a small and disinfranchised minority), and so the progressive positions of the national party had to be soft-pedaled or ignored, and sometimes openly contradicted by the expression of white supremacist ideology in the local press. These and other deviations reported by Burbank represent a thorough examination of political ideology as it was articulated at the gross roots and this examination of ideology is the core of the book.

The domination of heterodox elements in the local ideology may be overemphasized, however, for other data used by Burbank, precinct-level voting data, suggest that the Marxian conceptions of class struggle had taken root; the data clearly show a sharp division in the socialist share of the vote, the party doing well in the rural precincts and poorly in the county seat towns. Burbank also shows the political divisions between town and country persisted after the demise of the Socialist party itself. In the 1920s the Ku Klux Klan dominated the politics of many states, including for a time, Oklahoma. The Klan was a middle-class and small-town movement which did not attract the support of the rural folk by its appeals to patriotic chauvinism and racial identity. Indeed the rural areas were the most bitter opponents of the Klan, which suggests that the small farmer and tenant had learned and remembered who their enemies were. Perhaps the socialist experience in Oklahoma had a more profound impact on citizen attitudes than Burbank is willing to concede.

In summation, *When Farmers Voted Red* is a significant contribution to our understanding of American radicalism. As in the pioneering work of Walter Dean Burnham, there is a need for a critical examination of the political past, and monographic studies like the one reviewed here make a large step in the right direction.

JERRY W. CALVERT

Montana State University

The Visible Hand: The Managerial Revolution in American Business. By Alfred D. Chandler, Jr. (Cambridge, Mass.: Harvard University Press, 1977. Pp. xvi + 608. $18.50.)

The Visible Hand of which business historian Alfred D. Chandler, Jr. writes is not that of the federal government as regulator of business (as a political scientist might assume); it is that of the salaried manager of the modern multi-unit industrial corporation. His thoroughly documented study traces the replacement of market coordination of economic life by administrative coordination from the 1850s through 1917 in great (and often fascinating) detail, and ends with a summary characterization of "managerial capitalism" as we know it today: the supremacy of oligopolistic firms in coordinating flows of goods and services and allocating resources for future production, and the supremacy of the salaried manager—rather than the owner—within those firms. Chandler writes of the federal government as a "coordinator and allocator of last resort" since the Depression and World War II; but his thesis is that the rise of managerial capitalism is an organizational response to private sector economic need (which in turn owes much to technology), and he focuses entirely upon the development of the historical record within the business system, rather than upon that system's interrelationships with the *other* visible hand; i.e., the public one.

Although this book is therefore not directly concerned with economic-political relationships, it is nonetheless of very considerable interest and value from the perspectives of both political economy and public administration.

As all business regulation texts stress, the interstate character of rail transportation made it one of the earliest candidates for national regulation. It seems not to be simply coincidental that the railroads were also the initial locus in which economic coordination came to be exercised by an administrative hierarchy. As Chandler relates it,

> Technology made possible fast, all-weather transportation; but safe, regular, reliable movement of goods and passengers, as well as the continuing maintenance and repair of locomotives, rolling stock, and track, roadbed, stations, roundhouses, and other equipment, required the creation of a sizable administrative organization. It meant the employment of a set of managers to supervise these functional activities over an extensive geographical area; and the appointment of an administrative command of middle and top executives to monitor, evaluate, and coordinate the work of managees responsible for the day-to-day operations.... Ownership and management soon separated... (p. 87).

The same forces, I infer, stimulated both multi-unit management and new modes of control, on the one hand, and a set of business policies that could be matched on the shippers' behalf only with the aid of another developing bureaucracy: the one in Washington. In this respect, as in others, the private sector led the way and governments followed; i.e., from an organizational standpoint, the role of the middle manager was defined in the corporation, and then adopted in the public sector. One's understanding of both an old classic (Weber's) and a recent one (Dahl and Lindblom's *Politics, Economics and Welfare*) on the hierarchic mode of governance is enriched by the many case studies this book provides of such firms as General Electric and Du Pont (for the study of top management), and American Tobacco and Singer (for the study of middle management).

Another considerable value of *The Visible Hand*, from a political economy perspective, lies in its concrete, specific, factual information on the rise of the modern corporation; on the divorce of ownership from control, and on the coordinative role of the large firm. These are all topics on which there has recently been a tendency toward tendentious ideology as a substitute for analysis, and Chandler provides a substantial corrective.

MICHAEL D. REAGAN

University of California, Riverside

Class and Community: The Industrial Revolution in Lynn. By Alan Dawley. (Cambridge, Mass.: Harvard University Press, 1976. Pp. viii + 301. $17.50.)

In this Bancroft Prize-winning volume from the Harvard Studies in Urban History series, Alan Dawley imaginatively recreates the dynamic transformation in the social, economic, and political life of Lynn, Massachusetts, unleashed by capitalist industrialization in the nineteenth century. A Marxist writing in the tradition of British historian E. P. Thompson, Dawley sharply details the impact of industrialization on class relations within the Lynn community.

Shoemaking, by 1800 the mainstay of Lynn's economy, was a household industry under the control of master artisans. Within this system, Dawley observes, a rough equality and interdependence typified class relations. By the 1830s, however, Lynn's merchant-manufacturers had gained control of the shoe industry through institution of the central shop. The authority of the merchant-manufacturer rested not on labor but on ownership of property in the form of raw materials and the finished product. The older, more personal, relationship of master artisan and journeyman was overwhelmed as an employer-employee relationship based solely on wages promoted class inequalities.

According to Dawley, the 1860s marked a turning point in class relations in Lynn. Mechanization of the shoe industry transformed the city's economy into one based on factory mass production. Moreover, the manufacturers, through their membership in the Republican party, came to assert administrative control over the city. Thus, the end of the Civil War found Lynn's industrial capitalists masters of the city's economic and political life.

Dawley dramatically describes the militant response of Lynn's workers to these changes. Beginning with the formation of a Workingmen's party in the 1830s, the city's workers celebrated an "equal rights" tradition that evoked the preindustrial community of interdependent artisan households. From the Great Strike in 1860 to 1890, Dawley traces a repeated pattern of major strikes by Lynn's workers. Each strike aroused police intervention leading, in turn, to the political efforts of workers to elect a new mayor and oust the chief of police. Although workers succeeded each time in their immediate objectives, the lack of further reform once they controlled city government raises for Dawley a critical question—why were Lynn (American) workers unable to transmit their economic militance to the political sphere? Dawley believes that, uniquely in the United States, widespread participation by workers in the electoral process channeled their radical impulse away from a critique of the political system as a whole. The unity of workers and their bosses against the "Slave Power" in the Civil War, Dawley asserts, further stunted the development of a political class-consciousness among workers. Overall, Dawley contends that the "ballot box was the coffin of class consciousness" (p. 70) in the United States.

By insisting on Lynn as a "microcosm of the Industrial Revolution" (p. 8) in the United States, Dawley underestimates the diversity of industrial development in the nation. It is safe to assume that industrialization in Pittsburgh, for example, which not only occurred later in the century and encompassed greater variety and scale of industry but also involved its own complex ethnic and class relations, differed significantly in its impact on that city from the effects of the same process in Lynn. More importantly, Dawley imposes a narrow definition on how politics operate within the community. It was not simply the "suffocating embrace of wartime unity" (p. 195) that tied workers to the dominant culture. Pervading the community's institutional and social fabric—of which workers were a part—was the idea that a harmony of interests existed between the employer and employed in a free labor society. Moreover, while Dawley is correct in arguing that the political system generated mechanisms for resolving class conflict, the means that existed went beyond merely the ability of the electoral system to provide upward social mobility to a few working-class leaders as he claims for Lynn.

In this study of Lynn, Dawley has clarified many significant issues. If in the end he is not able to give us the synthesis of industrialization's impact in the United States during the nineteenth century that he hoped to, Dawley has, nonetheless, made a worthy beginning.

BRIAN GREENBERG

Princeton University

Counsel for the United States: U.S. Attorneys in the Political and Legal Systems. By James Eisenstein. (Baltimore: Johns Hopkins University Press, 1978. Pp. xi + 264. $15.00.)

United States attorneys are the representatives of the Department of Justice in the 94 federal judicial districts. With their assistants they handle criminal and civil litigation for the federal government, and they play a central role in shaping federal legal policy. Despite their importance, however, they have been the subject of relatively little scholarly research. This book, an extension of Eisenstein's previous work, substantially increases our understanding of these policy makers.

The book is not intended to be a comprehensive examination. Rather, Eisenstein seeks primarily to ascertain the role of U.S. attorneys in the policy process by analyzing their relationships with other actors: administrative colleagues and superiors, judges, clients in the federal government, and private attorneys. The book gives particular attention to the relationship between U.S. attorneys and the Washington office of the Department of Justice as a case study of a field office system in the federal executive branch. Eisenstein occasionally moves outside this focus on U.S. attorneys' external relations, most notably in a useful chapter on their backgrounds and selection. But some important aspects of their work, such as the internal politics of their offices and the criteria for prosecution decisions, are given only secondary attention within the context of the book's primary concern.

The study's major data source was a set of semi-structured interviews with policy makers, conducted primarily in 15 federal districts and in Washington. The analysis of the interview data is necessarily unsystematic. For this reason the study might have profited from more extensive use of the official statistics of the federal court system as sources of data on U.S. attorneys' litigation activity. Flawed though they are, these statistics would have provided the basis for some systematic analysis of issues such as constituency influence on prosecution decisions. But whatever weaknesses result from the reliance on interview data are minimized by the sensitivity with which these data are analyzed.

Eisenstein examines the relationships between U.S. attorneys and other actors from several perspectives. He gives special attention to the relative strength of the two parties in making policy and to the factors that determine their strength. He finds substantial variation in the power of U.S. attorneys' offices in relation to the Justice Department and district judges. He explains this variation largely in terms of district characteristics, particularly the size of the field office, but U.S. attorneys' own characteristics also help to determine their power.

The study also produces a broad range of significant findings on other issues. One issue of particular importance is the character of plea-bargaining in federal cases. In a short but insightful analysis, Eisenstein concludes that the defense holds a relatively weak position in bargaining at the federal level; he explains this weakness in terms of limited case pressures, the strength of the average case handled by federal prosecutors, and other factors. Findings on several other subjects, such as the cooperation between U.S. attorneys and district judges, will be of considerable interest to most students of judicial politics.

The book also merits the attention of other scholars interested in the policy-making process. As a study of policy implementation it constitutes a solid contribution to the growing body of empirical work on that subject, and it is especially useful in illuminating problems of central control over implementation activities in a field office system. The discussions of U.S. attorneys' relationships with judges and investigative agencies are relevant to scholars concerned with interinstitutional relations in government, in part because Eisenstein carefully identifies the factors that shape these relationships.

The book is enhanced by Eisenstein's use of conceptions from the literature on bureaucratic behavior to explain some findings. In employing these conceptions he underlines their utility in the analysis of policy-making processes. In this respect the analysis of conflict between U.S. attorneys and other actors is particularly valuable. Eisenstein indicates the importance of quantitative criteria for success as a basis for divergent incentive systems that contribute to conflict, and he shows the significance of tensions between generalists and specialists in the relationship between field offices and Washington within the Justice Department.

In a concluding section Eisenstein assesses the performance of U.S. attorneys' offices. He argues that the offices suffer from the inexperience of assistants and suggests a restructuring of salary scales to encourage assistants to stay in their jobs for longer periods. Given the depth of the research on which this proposal is based, it merits serious consideration.

LAWRENCE BAUM

Ohio State University

Medicare: The Politics of Federal Hospital Insurance. By Judith M. Feder. (Lexington, Mass.: D.C. Heath, 1977. Pp. ix + 177. $16.00.)

Judith M. Feder's *Medicare: The Politics of Federal Hospital Insurance* is a good book. It is lucid, tough-minded, and bristling with good judgments. Moreover, I liked her style. The book is written in the lean, sinewy prose without adornment or hyperbole. It belongs to the genre of implementation studies—that is, the book is directed to the output rather than the input side of government. The book should be considered, if one has a taste for classification, as belonging to that new but growing category of studies that seeks to explore policy results, the political forces shaping an agency's strategy in carrying out legislative intent, and the feedback links between program officials, clientele, and the wider political system. This is done, I should hasten to add, without a self-conscious or cloying conceptual apparatus. The logic of the study emerges clearly in the structure of the chapters and in the lucid development of the argument.

The objective of the study can be stated simply: how did the Social Security Administration, in implementing Medicare, choose the strategy that it did, rather than some other strategy, to carry out the intent of the legislation? The SSA as a check-writing social insurance agency, could emphasize simply getting the system established and functioning smoothly. Such a choice would put a premium on minimizing potential political conflict and would seem to assure program survival. Alternatively, the SSA could choose to use its leverage to effect the quality of medical care more assertively as well as to take steps to hold down the likely cost escalation that would follow from the influx of Medicare reimbursements to hospitals. SSA, in Feder's view, clearly chose the former, emphasizing program survival and accommodation with the major clientele groups at the expense of playing a more aggressive role in improving the quality of health care and in containing the rapid increases in hospital costs.

Specific topics treated include the early efforts to establish minimum quality standards for hospitals' eligibility for Medicare reimbursements which involved SSA reliance on outside institutions and compromise decisions reflecting the judgments of affected professional groups; the development of the "utilization review committee" system to monitor quality of care, an innovation in peer review and self-policing by the physicians; adoption of initial principles for reimbursing hospitals for Medicare patients and the subsequent consolidation of the system of reimbursement along lines that were favorable to the hospital. The reimbursement formulas were particularly favorable for the capital costs of hospitals, thus allowing expansion of facilities which in turn increased the base cost of patient care. Potential conflicts were typically averted by compromises which placated the health care providers but contributed to further escalation of costs. Finally, a lengthy chapter is devoted to the SSA's efforts to contain hospital costs once pressures began to arise within the political system, principally from Congress, and the issue no longer could be avoided. Here the argument loses some of its force. Feder blames a lack of significant progress in cost containment on the SSA's narrow "regulatory" approach which stressed payments limits and eschewed greater reliance on imaginative strategies such as co-insurance features (i.e., requiring consumers to pay a larger fraction of costs as their income permitted) and on changes in the market structure for medical care (health maintenance organizations and various forms of pre-paid group health coverage). This is too pat a solution. SSA was justified in having a cautious attitude toward such "cures." Moreover, this was a period of rapid inflationary pressure in the economy, which added greatly to the complexity of cost containment under Medicare and to which Feder devotes insufficient attention. My own reading of this period is that cost containment had become an urgent objective by the early 70s but that no one inside or outside of SSA really knew how to achieve the goal. It remains to be seen whether HCFA can do much better.

There are a few other minor quibbles I have with Feder's analysis, mostly relative to ambiguities at certain points in the argument. She recognizes that Medicare at the time of its adoption was not a health measure; rather, it was a payment measure designed to ease the financial burden of old people getting sick (especially the burden on children and relatives). But at times Feder seems inclined to blame SSA administrators for lacking the foresight and anticipation of problems that virtually no one had at the time. My own sense of the early period is that there was not quite the self-conscious political calculation that she at times seems to impute to the Medicate administrators—the politics were, after all, rather muddy and many players had conflicting interests—and rather more of a preoccupation with the details of getting a massive administrative undertaking established. She recognizes this as a substantial feat, but it is perhaps more of a feat than she acknowledges.

Similarly, there is a slight ambiguity in Feder's treatment of how much freedom of action she thinks administrators have under even broad statutory guidelines. At times she talks as if the statutory framework of Medicare were almost entirely irrelevant as constraints on administrative action, and seems to imply that SSA could close hospitals, reorient accounting systems, alter benefits, and enact a host of other measures at will. In her conclusion, Feder returns to the, I think sound, assessment that "altering the course of Medicare policy . . . rests less with administrators than with the Congress and the executive to which they are responsible" (p. 156). Finally, I miss any recognition that quality control and cost control may be quite different, and may, indeed, be conflicting objectives. The analysis acknowledges that quality control may be a more complicated matter or at least one less directly within the confidence and responsibility of the SSA than containing hospital costs, but yet at other times quality control and cost control are talked about as closely related if not in wholly interchangeable terms. One might argue that changes in the structure of the medical industry that improve the quality of medical care could also control the costs more effectively than current practices, but this is a point which has to be argued. These are minor flaws in an admirable book. I hope Feder keeps on working in this area, and gives us before long a cogent analysis of the recent Califano reorganization.

BRUCE L. R. SMITH

Columbia University

Home Style: House Members in their Districts. By Richard F. Fenno, Jr. (Boston and Toronto: Little, Brown and Co., 1978. Pp. xvi + 304. $6.98, paper.)

Introducing his book, Richard F. Fenno, Jr., tells of a representative who flies in from Washington for an evening's electioneering in the district. The representative is elated: he has just completed a lengthy phone negotiation with the president, culminating an exciting legislative struggle in which the representative has played a key part. Yet this is set aside as the legislator gulps down his dinner and hurries to a campaign appearance laden down with hundreds of shopping bags emblazoned with the legislator's name and picture. This is not, Fenno explains, a book about the world of Washington politics; rather, "it is a book about the world of shopping bags" (p. xii).

There are two Congresses, not just one. There is the familiar Congress of the textbooks—of lawmaking, investigating, or voting on Capitol Hill. But there is another Congress—the Congress of more than 500 disparate constituencies, many of them removed from the Nation's Capital by thousands of miles and by light years of perspective. To most of us, this second Congress is terra incognita. It is not easily researched, for it is geographically scattered and reflects the confusing diversity of the American scene.

As might be expected, Fenno proves an incisive guide to this barely charted territory. His book is "an exploration" (p. xi); and his research strategy is one of "soaking or poking" (p. 249).

The author accompanied a "sample" of 18 members (10 Democrats, 8 Republicans) on numerous district trips between 1970 and 1977. Although the open-endedness of this strategy may bother those attracted to formal methodologies, it is ideal for exploration—for soaking up impressions and generating hypotheses.

Fenno's conception of constituency is multifaceted. He suggests a nest of concentric circles, the largest of which is the formal Geographic Constituency. Less inclusive definitions include the Reelection Constituency (the supporting coalition); the Primary Constituency (hard-core supporters); and the Intimates (closest advisors and confidants).

To cope with these constituencies, legislators adopt "home styles" which are somehow linked to their personalities, backgrounds, constituency characteristics, and resource allocations. Fenno postulates that exchanges of trust, not congruences on issues, characterize the process of representation. Fenno has shifted the focus from *representation* to *presentation*. The shift is momentous, but accords with our emerging knowledge of voters' premises in assessing elected officials. As Fenno states, "it is the style, not the issue content, that counts most in the reelection constituency" (p. 153).

Three aspects of "home style" are probed: allocation of resources (time, staff); presentation of self; and explanation of Washington activity. Data on resource allocations of all members (not just those in the "sample") are somewhat inconclusive. The frequency of district trips varies directly with the district's closeness to Washington, and with whether the member's family remains in the district. District staff resources vary by region and state. Few explanations are available. Perhaps local traditions are responsible—such as the tradition of errand-running in old machine areas, or the use of state-level district staffs (as in California). Surprisingly, electoral safeness or vulnerability does not seem of overriding importance. More research will be desirable, especially in view of the increments in incumbents' perquisites authorized during and just prior to the period of this research.

To illustrate various types of "home styles," Fenno introduces us to various types of legislators—the Home Folks Congressman, the Issue-articulating Activist, the Errand Boy, the Local Political Leader, and others. Abounding in insights, these portraits will interest general readers as much as they do political scientists.

Readers will be impressed, as Fenno was, by the legislators' physical stamina, competitiveness, and sense of perspective. Readers may also be disabused of the currently fashionable dictum that the all-consuming goal of politicians is reelection. Fenno's subjects do, of course, fight for reelection (though some, like increasing numbers of their colleagues, give up and retire). Sometimes, however, they fight for the freedom to pursue other goals—policy making or Washington influence, for example. Politicians, it seems, do not live by reelection alone. However, the notion of a legislator's latitude, or freedom, needs further refinement.

Observers of the congressional scene will not be startled by the range of strategies used to explain Washington behavior to the folks back home. Reassuring is Fenno's testimony that politicians tend to give the same account of themselves, no matter what group they are talking to. (Inconsistency, after all, is mentally and psychologically costly.) What is disturbing, however, is that when legislators explain their behavior they often belittle Congress as an institution—a far cry from the institutional loyalty displayed a decade or so ago. "Members of Congress run *for* Congress by running *against* Congress," Fenno writes (p. 168). "The strategy is ubiquitous, addictive, cost-free, and foolproof."

More detailed reportage about the districts, and the author's experiences in them, might have been desirable. Constrained by the strictest standards of anonymity, much of Fenno's discussion of district styles is strangely disembodied, lacking a vivid sense of place or time. Most fascinating, in my judgment, were Fenno's observations about his own access to, and acceptance by, his subjects (pp. 259–79). Useful also were comparisons of how concrete issues (Nixon's impeachment, the 1970 Voting Rights Act) were handled by diverse legislators, some of whom were outside the "sample" and speaking for the public record.

The legislator's personality is another factor that impinges time and again upon the study but is not addressed in detail. Fenno rightly eschews pop psychiatry in analyzing his subjects, but more focus upon the psychological dimension—for example, Congress member B's sexual fixations, Congress member E's interpersonal ambivalence—might have yielded more clues about the selection of home styles.

In short, Fenno focuses upon the intersection of member-constituency relations: "home styles," as manifested in constituency encounters. From his observation Fenno has spun enough speculations to keep the entire cadre of congressional scholars busy for years (no mean achievement). We must now reach back into the funnel of causality, so to speak, to gain more precise knowledge of constituencies themselves and the interaction of politicians' personalities in producing these generalized roles which Fenno terms "home styles." And we must reach forward to explore Fenno's speculations on the consequences of "home styles" for representative institutions and representative government.

In this superb guidebook to the "world of the shopping bags," Fenno once again confirms his reputation as one of the best, perhaps *the* best, scholar of American politics. Quite properly, his book is reminiscent of those ancient explorers' maps, detailed in places but containing large blank spaces marked as "unknown territory." The test of Fenno's perspicacity will be how rapidly, and how fruitfully, his colleagues rush to fill in those voids.

Fenno's appendix on methodology is one of the book's most appealing features. For those who think that "data" are exclusively quantitative, Fenno's scrupulousness about every phase of his research ought to be revelatory. His personal credo for maintaining the purity of his access to subjects (pp. 261–63) may be too demanding for those of us who are impatient for involvement; but we cannot help but respect the scholarly integrity that underpins Fenno's choices.

ROGER H. DAVIDSON

University of California, Santa Barbara

Education and the Presidency. By Chester E. Finn, Jr. (Lexington, Mass.: Lexington Books, D.C. Heath, 1977. Pp. xv + 153. $15.00.)

Educators have traditionally regarded the political process with some skepticism. It was believed that the political arena was dirty and inherently contaminating. In recent years, however, as it has become clear that what happens in Washington and in state capitals greatly influences what happens on the campus or in the classroom, the education-politics relationship has received increased attention from scholars. Chester Finn's *Education and the Presidency* is the latest among a number of first-rate studies that focus on the intersection of the realms of education and politics.

This study is on an aspect of the policy process that has been virtually ignored—the formulation of presidential policies and programs. In Finn's words, the study examines "the interplay of ideas, analyses, personalities and procedures that produce presidential policies" (p. 3). Although the author focuses on education, there is a good deal of material that will be of general interest to the student of public policy. The specific targets in this case are the events that shaped the Nixon administration's 1970 special messages on educational reform and higher education. Many of Nixon's specific proposals in these two messages later became part of the landmark education amendments of 1972.

According to Finn, the primary vehicle for the development of both messages was the "working group," a committee chaired by a presidential assistant and composed largely of political appointees and their aides. The Nixon approach resembled that of other presidents, with some "differences of degree." Finn notes: the president relied on these groups "heavily if not exclusively," they "systematically excluded" those who might not put the president's interests first, the working groups were an arm of the president's own staff structure rather than an extension of the cabinet, and finally, the groups were very secretive (pp. 6–7).

Perhaps the most troubling aspect of the process as recorded by Finn, then a staff assistant to the president, is its closed nature. Unlike the Johnson White House which solicited outside opinions in policy formulation, the Nixon administration "rarely sought the participation of career officials, interest group spokesman, or denizens of Capitol Hill, even those of their own party" (p. 92). Despite its limitations, Finn finds the working group a valuable tool in presidential decision making, one that "foster[s] speed, orderliness, and confidentiality" (p. 119).

Part of the difficulty in analyzing presidential policy making is, as Finn notes, that it is largely shaped by the decision-making style of the person who occupies the Oval Office. How the president likes to receive information and the amount of procedural and substantive conflict a president is willing to negotiate all influence the executive decision-making apparatus. President Nixon's distaste for any form of conflict and his well-publicized desire for solitude and secrecy in decision making made the working group approach described by Finn an efficient and comfortable arrangement. Another president might regard the Nixon approach as overly confining. Thus while Finn's book provides valuable information about the elements that influenced and shaped presidential decision making in the Nixon White House, the process is likely to vary considerably from president to president and generalizations must be drawn with care.

Education and the Presidency is well written and a welcome contribution to the literature of education and public policy. It should interest those concerned with the presidency, public policy decision making and education. For those interested specifically in the education amendments of 1972, this book, along with Lawrence Gladieux' and Thomas Wolanin's companion volume, *Congress and the Colleges* (Lexington Books, 1976), will provide an indispensible starting point.

TERRY W. HARTLE

George Washington University

Congressional Staffs: The Invisible Force in American Lawmaking. By Harrison W. Fox, Jr., and Susan Webb Hammond. (New York: Free Press, 1977. Pp. xiv + 227. $12.95.)

If one were to visit any of a number of capitals in the United States and ask someone there what has been the most important change in the legislature during the last few decades, the likely response would be a single word—staff. Walking about the capitol building, an observer would get a number of visual confirmations from the plethora of staff offices. Staff members today are inexorably involved in all aspects of the legislative process.

Yet, staffing as a factor in the process of legislation has been, until fairly recently, almost completely ignored by political scientists. In the last few years, several papers and articles have provided the outline for a more complete study of the actual backgrounds, roles and functions of legislative staffs. But this book, by Harrison Fox and Susan Hammond, is the first to focus on the professional staffs of Congress since Kenneth Kofmehl's pathbreaking effort in 1962.

Based on the authors' dissertations and several years of experience on Capitol Hill, the book is a useful one. It is an essentially descriptive account of the operations of the professional staffs of the Congress. The authors trace the historical growth of professional staffs into a "congressional bureaucracy," employing over 38,000 persons. Unfortunately, while using the term "bureaucracy" (p. 10) to encompass the professional staffs which have developed in Congress over the last few years, the authors do not thereafter invoke various aspects of organization theory or public administration as salient concepts. This is unfortunate since it has been argued that public administration is a logical and relevant field within which to deal with the subject of legislative improvements and the role of the professional staff.

The authors present an overview of staff characteristics and sketch a "profile" of staff aides. However, they do not compare their data with Kofmehl's so that we might have some indication of what changes have occurred over the years. For example, has the expansion of congressional staff changed recruitment practices? Are staff becoming more partisan? Are staffers with training in the social sciences supplanting lawyers as Hill aides? Instead, the findings are presented seriatim, with the understanding that "attributes of staff presumably affect congressional operations" (p. 33). Moreover, no attempt is made to relate these data to the findings on staff activities.

The responses of Senate personal staff members to questions about what activities they were engaged in were subjected to a factor analysis to yield what the authors designate "general professional staff roles." While the use of the concept of role is suspect here, the outlines of a potential research typology do seem to be visible. This potential is marred, however, by a proliferation of role types: personality and group types (p. 39); career orientation role types (p. 64); the general professional staff's roles (pp. 93–95); and finally patterns of role types (pp. 95–96).

While Fox and Hammond describe exhaustively the formal aspects of congressional staffing, the more interesting problems start where their analysis ends. For the work is limited in regard to the most important issue from the perspective of politics—the impact of professional staff activity on congressional decision and policy making. The chapter on staff impact is one of the briefer ones and is composed of a summary of previous sections on staff backgrounds and several quotes from legislators. Staff size is mildly related to committee output (number of hearings, the proportion of bills reported). This is not surprising since imaginative staff aides often tend to create—or at least attract—heavier burdens. But what part does staff play in the actual shaping of policy? Under what conditions do staff members influence legislative deliberations? Is there any danger of large staffs developing into independent power centers and usurping legislative roles? Given the wealth of data previously presented and the authors' obvious knowledge, this chapter is rather disappointing.

There are several annoyances. All the tables are in an appendix. There are several misspellings and typos (turnnover, p. 8; pesonalized, p. 60; imediate, p. 79; etc.) and incorrect references—Fishel is Fischel; Wilensky is Wilonsky; Kovenock is Kovenoc; Musolf becomes Muloff.

Despite the limitations noted, the book is an important one. Fox and Hammond have provided an up-to-date, in-depth description of the professional staffs in Congress. These staffs have grown considerably in recent years; some would argue that they have become a force almost as powerful as the elected Congress. The authors have contributed to increased understanding of the legislative process and shed light on the roles and impact of the professional staff. Further research is necessary before we can determine what difference these staffs have made, but it will have to speak to the agenda generated by this work.

ALAN P. BALUTIS

Department of Health, Education, and Welfare

Policing a Free Society. By Herman Goldstein. (Cambridge, Mass.: Ballinger Publishing Company, 1977. Pp. xi + 371. $16.50, cloth; $7.95, paper.)

Rare is the book which presents both a balanced assessment of developments in a field and a comprehensive new framework for understanding that field. Herman Goldstein has achieved both for police in America. His thorough examination of fundamental issues begins with the observation that "police, by the very nature of their function, are an anomaly in a free society. They are invested with a great deal of authority under a system of government in which authority is reluctantly granted, and when granted, sharply curtailed" (p. 1). Goldstein shows that the nature of police tasks requires that this authority be exercised by individuals at the operating level. Further, police authority is ambiguous and surrounded by a jumble of contradictory expectations. Statutes usually require police to enforce all the laws all the time, yet the public will not tolerate full enforcement of many laws. The public holds police responsible for preventing crime, yet police have no influence on the social factors which promote the many varieties of unlawful behavior. The book elucidates these and myriad similarly complex discrepancies. The question guiding this work is: how can policing be restructured so that police power more adequately serves the public good as defined by constitutional safeguards and democratic processes?

Chapter by chapter Goldstein builds the framework that provides answers. He analyzes in the first four chapters what police actually do and sympathetically explains their problems in doing it. A pervasive problem is that the present structuring of the job requires duplicity. The next four chapters weigh the issues of power—the structuring of discretion, the political direction of police, the control of police misbehavior, and the handling of corruption. The last four chapters not only explain the minimal impact of recent attempts to improve police organization through developing leadership, upgrading personnel and increasing education, but also suggest realistic alternatives.

Goldstein brings to his task the mature reflections of 20 years' experience with police. His perspective is that of an administrator steeped in constitutional law and conversant with social science research. He suffers no illusions concerning the entrenched nature of police abuses and the difficulty of police reform, having worked four years in the Chicago police department as right hand to the most influential police manager of the mid-century, O. W. Wilson. Since 1965 he has served at the University of Wisconsin law school.

Goldstein discourses fluently upon the whole literature of police, the uneven nature of current practices, and relevant court decisions. The prolific citations to empirical studies, primary documents, and reflective essays provide a comprehensive summation of the literature of the police field.

The book is socratic; reasoned argument carefully examines the complexities of each issue and then leads inexorably to conclusions that refute prevailing opinion. The doctrines which Goldstein destroys are those derived from the major police reformers of this century: that crime is the paramount responsibility of the police, that organizational upgrading is a sufficient goal for reform, and that police power can be properly controlled through a centralized structure that is insulated from politics.

The new framework by which Goldstein addresses the enduring problems of policing rests on a view of police as an agency of municipal government housing a multitude of functions. In listing the appropriate objectives, he draws upon the American Bar Association standards adopted in 1973, of which he was the original draftsman. They are: to prevent and control conduct threatening to life and property, to aid individuals in danger; to protect constitutional rights; to facilitate traffic; to assist those who cannot care for themselves; to resolve conflict; to identity incipient problems;

and to create a feeling of security. A recognition of the full range of complex services which police provide highlights the inadequacy of the standard goals of efficiency stated in terms of response time, arrests, and clearance rates.

Discretion is essential for personnel who carry out tasks with such complex multiple objectives. Which objective takes priority in a given situation? What method of intervention is most likely to achieve the objectives? What form of disposition is most appropriate? The present police organizational arrangements are clearly inadequate for controlling discretion. Once the existence of discretion is acknowledged, it can be structured and controlled through explicit policies.

The denial that police make policy results from the reform efforts to insulate police from the politics of patronage, favoritism and corruption. Consequences of this apolitical posture have been lack of accountability and a highly impersonal service that underlies the estrangement between police and public. A balance is required between the need for communities to influence policy and the need for police managers to run their departments. Individual officers need both guidelines for the exercise of discretion and assurance that when they properly enforce the law they will be safe from retaliation, Goldstein observes.

The civil disorders of the late 1960s mark the beginning of our present period of challenge to police orthodoxy by citizen protest, the isolated efforts of individual police managers, and the explosion of research. Goldstein's book fully comprehends and adds to that challenge. Researchers who use Goldstein's framework to shape their hypotheses will be addressing the fundamental issues of policing a free society.

DOROTHY GUYOT

Center for Policy Research, New York

Black Separatism in the United States. By Raymond L. Hall. (Hanover, N.H.: University Press of New England, 1978. Pp. x + 306. $15.00.)

The appearance of the first extensive treatment of black separatism in America since Theodore Draper considered it as part of his *Rediscovery of Black Nationalism* in 1970 should be cause for elation. The rejoicing should be even more unrestrained when, rather than taking Draper's general and popular approach, a writer with scholarly credentials promises a disciplined and theoretical approach to this most persistent and hardy varietal of the nationalist stock. We are drawn, even more forcefully, to a book which promises that it is enriched by "personal interviews and correspondence with most of the leadership . . . information gleaned from primary and secondary sources . . . [and] information gained from individuals who defected from certain organizations" in addition to the author's own impressions as a participant-observer in these separatist groups. What emerges from Raymond L. Hall's *Black Separatism in the United States* is a reality which falls short of the promise.

Unless Hall's primary source materials have been woven too skilfully and seamlessly into the narrative to be recognized, I was able to identify only three documented instances of such primary materials: a conversation with James Farmer in 1968, a quotation from a letter from Milton Henry, and a reference to "notes taken by me." I will certainly concede that the book is informed by the author's personal experiences, but if interviews, internal documents, and other primary sources are so conspicuously mentioned—not merely alluded to—the author ought to make note of them even if he must mark them "not for attribution." Without any specific evidence of substantial use of such sources, we are left with what appears to be the author's heavy reliance on the respectable, but all-too-familiar, works which are found in the secondary literature.

After a worthy recapitulation of the historical expressions of black separatism which runs to roughly 100 pages, Hall gets into the shank of his subject which is an examination of four groups which he denominates as separatist: the Nation of Islam, the Black Panther Party, SNCC-CORE, and the Republic of New Africa. Hall excludes from his separatist category such groups as Ron Karenga's US, and the group around Imamu Amiri Baraka whom Hall identifies as "Ameer Baraka." Karenga receives no mention at all, nor does the Black Panther Party of Northern California, the Bay Area group of cultural nationalists with which the Black Panther Party for Self-Defense did battle and with whom they struggled for the blessing and endorsement of Malcolm X's widow, Mrs. Betty Shabazz.

Hall makes it clear that he considers cultural nationalism analytically distinct from separatism as a modality of black separatism, but by his own admission the Panthers' "almost complete rejection of African heritage" (p. 125) and his citation of Huey Newton to the effect that even if blacks were to gain control of six southern states they could not compete with the capitalist remainder (p. 195), suggest that the Panthers' "revolutionary nationalism" was more revolutionary than nationalist. The Panthers look decidedly uncomfortable in the separatist company in which Hall has placed them. Their quarrel was with capitalist America, not white America, and it seems somewhat awkward to consign them to the same category as groups which favored self-segregation or emigration.

The original contribution of Hall's book lies in its useful typology of the kinds of personalities which are attracted to separatist movements, but since the bulk of the study is at the organizational level, the individual-level formulations seem disconnected from its main thrust. There is an interesting exploration of the relationships between the various separatist gropus, but it appears to derive almost wholly from official pronouncements and the secondary literature rather than from any startlingly original sources.

Despite the presence of a report on the current status of these groups, the study has a rather dated quality to it, but even the older literature is characterized by some glaring omissions. I do not think that the Panthers can be appraised fairly without some mention of the most notable book by a defector, Earl Anthony's *Picking Up the Gun* (Dial Press, 1970). *The Glass House Tapes* (Avon, 1973) which raises questions about the use by police of cultural nationalist groups to destroy the effectiveness of the Panthers, also suffers from lack of mention.

As a straight history of black separatism, however, *Black Separatism in the United States* is a serviceable and helpful book. Had Hall not primed us to expect a substantial amount of novel material, the book might have stood more firmly on its own as a handy and reasonable restatement.

ROSS K. BAKER

Rutgers University

Emergency Employment: A Study in Federalism. By Howard W. Hallman. (University, Ala.: University of Alabama Press, 1977. Pp. ix + 207. $11.50.)

Howard W. Hallman has written an unusual book. There is a fairly extensive body of literature documenting the failure of federal grant programs; Hallman tells a success story. In somewhat extravagant language, he describes the program for providing public service employment under the Emergency Employment Act of 1971 as "a new chapter in the continuing saga of American federalism."

The title of the book is misleading. The book is a case study of a single federal program. It does not explore the implications of the Emergency Employment Act either for federalism or for the administration of the federal system. In the main the book consists of a concise, straightforward narrative summarizing the legislative history of the Emergency Employment Act and the steps taken by the federal government and its state and local partners to carry out the public service employment program. The final chapters briefly evaluate program results.

If there are any lessons to be learned from the case study, Hallman does not explicitly identify them. He does not attempt to explain why this program was successful when those studied by Jeffrey Pressman and Aaron Wildavsky in "Implementation" and Martha Derthick in "New Towns in Town" were failures. What did the administrators of the public service employment program do right?

As is not true of most of the "great society" programs, the goals of the Emergency Employment Act were relatively clear-cut and accepted by all participants. The program was simple in design and comparatively simple in execution. As Hallman notes, the act was a "modified form of revenue sharing. The money was not entirely unrestricted because it was targeted at a specific objective, i.e., the hiring of the unemployed, with preference given to certain segments of the population. But many programmatic details were left up to the states, cities, and counties." Faced with a budget squeeze and layoffs due to cuts, local governments experienced little difficulty in identifying useful jobs to be funded by the program.

Whether or not the program was successful depends on the measures employed to evaluate results. Within two years 318,000 persons were given jobs, including 13,000 summer jobs for young people. The statistics are impressive, but there is considerable evidence of "creaming." As Hallman points out, "The high proportion of persons with high-school education and beyond and the preponderance of men aged twenty-two to forty-four suggests that the cream of the crop was selected much more frequently than the hard-core unemployed." Program evaluators found that non-disadvantaged minority persons were often classified as "disadvantaged." While the program did little to alleviate hard-core unemployment, it did provide useful jobs and enabled governments to avoid curtailment of essential public services.

Erwin C. Hargrove has urged that "policy and program analysis should be supported by new kinds of policy research to improve the quality of thought about program implementation." Case studies such as "Emergency Employment" are welcome because they contribute to our knowledge of program administration. In and of itself, the book does not add a great deal to our understanding of the factors that make for program success or failure.

HAROLD SEIDMAN

University of Connecticut

Plea Bargaining: The Experiences of Prosecutors, Judges, and Defense Attorneys. By Milton Heumann. (Chicago: University of Chicago Press, 1978. Pp. viii + 220. $15.00.)

Legal realists such as Karl Llewellyn, Jerome Frank, and Raymond Moley would have been delighted to read Milton Heumann's realistic account of the dispositional process. Winner (in its earlier form) of the 1977 Corwin Prize for best Public Law dissertation, this work is based on field research in 1972–73 with six Connecticut trial court workgroups to which Heumann unobtrusively gained access. He sat in each courtroom for a few days before approaching a junior assistant prosecutor and requesting permission to observe and, eventually, to talk over impressions of criminal procedures. Heumann subtly ingratiated himself to the point where soon he was allowed to tag along with members of the courtroom workgroup, was included in plea bargaining sessions, and was even asked for his opinions on particular cases. Becoming a participant-observer enabled Heumann to refer to particular cases and otherwise to function as more of a colleague than an outsider in the open-ended interviews which followed. Defense counsel soon clamored to be interviewed themselves, and finally the judges' cooperation was easily achieved. Three-quarters of the 71 respondents permitted Heumann to tape their interviews, and the transcripts of their frank and salty remarks liberally season this meaty *pièce de résistance.*

Of the six court settings studied, three were petty trial courts while the other three were the corresponding trial courts of general jurisdiction in the same locations. Two locations were urban and thus characterized by high crime rates and caseload pressures, but the remaining location was more bucolic and therefore lower in crime and caseloads. Heumann thus achieved a rudimentary balanced quasi-experimental design, with his two key variables being (1) the level of court (and categories of crime processed) and (2) caseload pressure. Caseload pressure proved to exert relatively low influence on plea bargaining, but level of court did affect plea bargaining styles. Heumann did not measure these effects rigorously; his analysis is for the most part impressionistic, and in such an exploratory study we should not expect more.

The report's guiding theme is adaptation: the process whereby new prosecutors, defense attorneys, and judges cope with the tawdry bureaucratic reality of the dispositional process. Gradually they shed their original preferences for trials and formal motions and their belief in the factual innocence of the accused. Eventually most members of the courtroom workgroup develop a preference for the negotiated plea, accepting it as more just, more flexible, as a more certain outcome. Their verbatim comments on how they adapted provide a searing indictment of legal education, such as: "I took one course in criminal law . . . I had ——, . . . and he didn't teach criminal law. He taught his version of Judy in the Sky with Glasses" (p. 49).

Heumann imposes a somewhat contrived distinction between what newcomers are taught and what they learn. Being taught involves rewards and sanctions which instruct the new workgroup members on how to act, whereas learning involves voluntary reconceptualization of the work environment unprovoked by the immediate application of rewards and sanctions (p. 53). But the two processes are only separable analytically and not empirically: respondents greatly confuse them. Furthermore, Heumann's concept of learning embraces newcomers' comparisons of the costs and benefits of the adversary and negotiated approaches (p. 100), which is certainly comparison of potential (if not immediate) rewards and sanctions. Heumann later presses the distinction into service when he theorizes that most newcomers learn to like plea bargaining and thus would adapt to it regardless of caseload pressures and other incentives and disincentives. A theoretically sounder approach might be decision or game theory, which indicates which alternative choices rational (or experienced) decision makers would pick to maximize given objectives under conditions of uncertainty or incomplete information. This conceptual lapse aside, most readers will be grateful that Heumann did not clutter his colorful report with mathematical models and payoff matrices, but instead concentrated on luring readers into the world of the courtroom bureaucrat. The vivid comments recreate the courtroom, or, more often, the bargaining table scene as richly as Dickens' depictions, as deftly as Daumier's sketches.

Heumann's suggestions for further research show his concern to expand sample size, but we should perhaps refrain from embarking immediately on the quest of the Holy Grail of Large Numbers. Small-sample methodologies, such as Stephenson's Q-methodology, amply explained by Steven R. Brown in "Political Literature and the Response of the Reader: Experimental Studies of Interpretation, Imagery, and Criticism" (*American Political Science Review* 71: 567–84) may hold far more potential for exploring courtroom bureaucrats' states of mind.

GREGORY CASEY

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Campaign Organization. By Xandra Kayden. (Lexington, Mass.: D.C. Heath, 1978. Pp. x + 182. $3.95, paper.)

Campaign Organization is an interesting but quite specialized addition to the burgeoning literature on contemporary political campaigns. Its limitations are that it focuses narrowly on a subject of limited usefulness, even for campaign specialists, and that it fails to relate itself to recent significant literature.

The book is based on Kayden's personal observations of the unfolding of three statewide campaigns in 1974—the Republican gubernatorial campaign of incumbent Francis Sargent in Massachusetts, the Republican gubernatorial campaign of Lamar Alexander in Tennessee, and the Democratic senatorial campaign of incumbent Birch Bayh in Indiana (the only victorious one of the three). Kayden spent several weeks observing each separate campaign, and at times the timing of the visits has strange episodic results, as when summaries of capital city television coverage of the three campaigns is restricted to September 18–27 for Tennessee, to October 3–12 for Indiana, and to October 14–24 for Massachusetts (pp. 127–30).

The preface notes that political campaign organizations are like other organizations, except that they are temporary and their work is characterized by heightened intensity. The author characterizes the book as "an analytical description of campaign organization and of the impact of the environment of those organizations on the people who participate in them." The book is process-oriented; Kayden is concerned with "how people make decisions rather than with what decisions were made."

The reader gets a comprehensive picture of campaign organization and of personal interactions on the three campaign staffs (the discussion of the relationship between Jack Flannery and Al Kramer of the Massachusetts campaign staff, pp. 32–39, is intriguing), but obtains little sense of how campaigns are planned and conducted or of the place which campaigns for individual candidates occupy in the political system. The specific topic of campaign organization is perhaps not worth a book, unless the book develops important questions, focuses suggestions for further research, or addresses more systematically the enduring problems of choosing among alternative approaches to campaign staffing, functional jurisdiction, hierarchical control, and the relationships among the candidate, manager, staff, and party apparatus.

Campaign Organization lacks a compelling and clear-cut organization or a careful enumeration of the essential characteristics of campaign organizations, the sorts of things I have referred to elsewhere as campaign variables—independence from other campaigns, personality-attitudinal characteristics, audience target, strategic planning, origin of resources, topical emphasis, campaign direction, and political insight, among others. Chapters or sections on such variables might have served as more effective vehicles for summarizing the author's observations. Chapter 5 is a good summary of basic campaign tasks (scheduling, research, speech-writing, polling, advertisement production), but the title ("The Organization as a Campaign") is not helpful. Nor is the inverted titling of chapter 4 ("The Campaign as an Organization") expressive.

It is striking that the book, which carries a 1978 copyright date, contains no reference to such campaign-specific research as that of Robert Agranoff, *The Management of Election Campaigns* and *The New Style in Election Campaigns;* David Leuthold, *Electioneering in a Democracy;* James M. Perry, *The New Politics;* Robert Huckshorn and Robert Spencer, *The Politics of Defeat;* Jeff Fishel, *Party and Opposition;* John Kingdon, *Candidates for Office;* Karl Lamb and Paul Smith, *Campaign Decision-making;* Marjorie Randon Hershey, *The Making of Campaign Strategy;* or Murray Levin, *The Compleat Politician.*

It is as if Xandra Kayden were working alone in an isolated and unplowed field. Perhaps this is why the book, while interesting and well written, leaves the reader with the feeling that it is insufficiently comparative, theoretical, and informative. On the other hand, the author has observed with discriminating attention the minutiae of three campaigns and has developed thoughtful conclusions that will need to be read by professionals who must understand the role of candidate organization in contemporary political campaigning.

ALAN CLEM

University of South Dakota

Pollution and Policy: A Case Essay on California and Federal Experience with Motor Vehicle Air Pollution, 1940–1975. By James E. Krier and Edmund Ursin. (Berkeley: University of California Press, 1977. Pp. vii + 401. $15.95.)

In the case of auto emissions policy, California, spurred on by the acute problems in Los Angeles county, led the rest of the nation. *Pollution and Policy* by James E. Krier and Edmund Ursin, law school professors at UCLA and the University of San Diego, is a history of California and federal auto emissions policy. After an excellent summary of the air pollution problem from scientific, engineering and economic perspectives, the authors devote the bulk of the book to a discussion of the era leading up to 1970 when the framework for present national policy was established with the passage of the 1970 Clean Air Act.

While the authors' careful scholarship and their attempt to be even-handed cannot be denied, questions can be raised about their use of history for analytical purposes. So much of what is considered public policy—books such as *Politics and Policy* by James L. Sundquist and *Regulating the Poor* by Frances Piven and Richard Cloward, are histories. This use of history for the purpose of policy analysis needs to be looked at more critically. Obviously it is true that "the failings of present policy to realize announced ends are due in part to the legacy of earlier efforts" (p. 307) as Krier and Ursin conclude, but such an assertion is a cliche and hardly a rewarding one for the reader who has taken the time to carefully read this book.

Like another recent history of air pollution policy (Charles O. Jones, *Clean Air: The Policies and Politics of Pollution*, which moves between policy development at the local level—in this instance Western Pennsylvania—and policy development at the national level) Krier and Ursin's focus on the California experience and its relationship to national experience is appropriate. However, the excessive detail of their coverage of events in California, although it does contain rewarding passages, is tedious reading. Moreover, the authors' discussion of the 1970–1975 period, the crucial era with respect to air pollution control, is less comprehensive and satisfactory than their discussion of earlier occurrences. Krier and Urwin would have served their readers better had they devoted themselves more to the interesting 1970–1975 period and concentrated less on the specifics of California policy in the pre-1970 period.

Krier and Ursin's two-chapter conclusion, which they bill as an appraisal, is provocative in that the authors' point of view comes to the surface. They conclude that policy moved too slowly before 1970 and too quickly after 1970; that crises, some real and some manufactured by the media, pushed policy making forward; and that policy makers "fixated" on technological and regulatory solutions while disregarding what Krier and Ursin believe to be a sane and reasonable approach to the problem—a tax on emissions. These conclusions, however, are not new.

Krier and Urwin's discussion of policy development by a process they label "exfoilation" is fairly interesting; but "exfloilation" might better be termed learning from experience, for it is nothing more than the gradual unfolding of policy in response to feedback from citizens, interest groups, and research scientists. What is fairly unique about their point of view is an insistence that the federal government retreat from its efforts to control emissions because of excesses like the EPA transportation plan that would have cut gas consumption in Los Angeles by over 80 percent. Krier and Ursin therefore look favorably upon a strategy that would differentiate between various regions, but they fail to sufficiently take into account the political and administrative problems that such a strategy would entail.

What mars this book is the authors' limited understanding of the task of writing history. They chronicle the events that occurred, but the writing of history is more than a chronicling of events. Had the authors adopted a thematic approach instead of a chronological one, their book would have been more penetrating and incisive. As it is, numerous interesting issues that they allude to are not systematically explored. It is a shame that the authors' view of history is so narrow because history can be a useful approach to policy analysis. *Pollution and Policy* is a good start but an authoritative work about auto emissions policy still needs to be written.

ALFRED A. MARCUS

University of Pittsburgh

Federal Government and Urban Problems. HUD: Successes, Failures, and the Fate of Our Cities. By M. Carter McFarland. (Boulder, Colo.: Westview Press, 1978. Pp. xviii + 277. $20.00.)

This is a clear and orderly description of the objectives and programs of the Department of Housing and Urban Development, with commentary on each, written by a long-time senior economist in the department and its predecessor, the Housing and Home Finance Agency. Cast in non-technical language, the book is characterized by breadth rather than depth. Even though it covers much less than the title suggests (its subject is only HUD, not the federal government as a whole), it can offer only 20 pages or so on each of the department's major purposes. Each chapter presents the rationale for a set of programs; describes their evolution, their accomplishments, and their present scope; then summarizes expert criticism and offers the author's own judgment and recommendations.

The reader will finish the volume with a better understanding of what HUD does and why, and will find McFarland's observations to be mature and balanced, and by no means defensive. But the specialist in policy analysis who is looking for the application of rigorous evaluation techniques will not find them here. The student of urban problems who seeks depth of analysis of any particular problem or program will find only introductory material in this volume. The student of policy-making processes or bureaucratic behavior who hopes to find here an insider's insight into how things actually happen in a big federal department will be disappointed; the author occasionally touches on incidents—reorganizations, for example—that would provide lessons for public administration but he does not develop them into illuminating case studies.

And every serious reader will be frustrated by the handling of footnotes. The publisher has gone to great pains, in the first place, to hide the references so they cannot be found unless one memorizes the number of the chapter one is reading (and who ever does that?). Then the author has compounded the difficulty—and the irritation—by citing only entire publications, without page numbers. To find the context for a quotation McFarland has used, the reader would have to search through anywhere up to 600–700 pages of the original source. Why, if footnotes are to be unfindable and unusable, do publishers and authors bother with them at all?

JAMES L. SUNDQUIST

The Brookings Institution

Innovation and Implementation in Public Organizations. Edited by Richard R. Nelson and Douglas Yates. (Lexington, Mass.: Lexington Books, D.C. Heath, 1978. Pp. xi + 186. $18.00.)

With its interdisciplinary focus on such timely concerns as innovation and implementation, this book will have immediate appeal to students of public administration. Two characteristics distinguish, and limit, the book's contribution to this field. For one, the editors have assembled in one volume nine brief case studies of innovation in a variety of institutional settings. Second, many of the essays are written by practitioners who draw on direct experience in the organizations under study.

With these essays as the empirical groundwork, Nelson and Yates hope that the book will point the way toward "the underlying and recurring patterns of innovation and implementation . . . found in the experience of different public organizations." Because of its peculiar case study format, however, the book largely fails to accomplish this objective in any systematic manner. In short, the use of disparate case studies, written with varying degrees of author involvement, results in a highly uneven treatment of the subject. Unfortunately, the editors fail to synthesize the material in an essay that points the way toward productive lines of future inquiry.

As a result, the reader is left with a potpourri of variables involved in innovation and implementation. In their brief introductory essay, Nelson and Yates define innovation as "doing something other than what is in [an organization's routine] repertoire." The nine case studies amply illustrate the range of possible applications of this definition—varying widely in the scope of innovation attempted, the manner in which innovation is introduced, the number of institutions involved in innovation, and the nature of the author's involvement.

In addition, the essays themselves are of uneven quality. Several are essentially descriptive, paying little heed to theoretically interesting concerns. Others generate theory-based findings but are hampered by deficiencies in the data presented. Peter Szanton's review of innovation in ten urban public services produces findings of questionable reliability, yet commendably he provides a theoretical framework for examining innovation experiences. One wishes that the editors had followed suit; at least it would have been helpful if they had used the case studies as inductive material from which to generate hypotheses for more systematic study. Despite their introductory disclaimer against "conceptual gymnastics," it remains a mystery why they chose to let the individual essays "stand on the strength of their particularity."

Although space does not allow sufficient treatment of individual essays, some are notable for their substantive insights into the innovation process. Douglas Yates' account of the New York decentralization experience points to an inherent contradiction between the logic of innovation (ideas that offer something for everyone) and the logic of implementation (realities that offer something for everyone) and the logic of implementation (realities that unmask conflicts disguised behind grand ideas). Malcolm Feeley's account of the New Haven Redirection Center for pretrail detainees suggests that this contradiction may result in part from the conflict between the "system perspective" of those initiating change and the "agency perspective" of those charged with implementation.

These and other obstacles to innovation are to some degree overcome in examples of relative success described in several essays. Two in particular—N. Dickon Reppucci and J. Terry Saunders' account of the Connecticut School for Boys, and Lawrence Sherman's review of neighborhood team policing experiments in six cities—provide compelling evidence of the impact of participation. In effect, participation in the initial planning stage by those affected during the implementation stage leads to more precise innovation objectives and greater clarity of anticipated benefits, resulting in fewer instances of what Murray Edelman has characterized as "words that succeed and policies that fail." This is particularly so in settings where coordination of the implementation stages is enhanced by centralized authority.

Insights like these are, however, hard to generalize to the range of innovation experiences recounted in the book—indeed, this difficulty may have led Nelson and Yates to forego a substantial organizing essay. One general difficulty is that many of the experiences focus rather narrowly on the institution undergoing innovation, thus avoiding many of the complicating political and economic factors that influence both the probability and scope of organizational change. It may be that innovation in a contained organizational setting may not be particularly comparable to more broad-based (and seemingly significant) innovation in the political or economic environment. On the other hand, the micro-organizational focus of several essays may produce lessons which can be generalized to broader political interactions.

In addition to the isolated insights of particular essays, the book as a whole makes two significant contributions to the literature. First, it seems most useful as an introductory reader—providing students with a range of innovation settings and methodologies used to study them. Simultaneously, although frustrating in their disparity, the everyday-life views of institutions undergoing change provide students with insights into organizational dynamics that are grounded in the real world. Having encountered this material, they may be in a better position to explore existing theories of innovation and to generate productive hypotheses for future study. One only wishes that the editors had done the same.

EDWARD P. MORGAN

Lehigh University

The Abuse of Power: The Permanent Government and the Fall of New York. By Jack Newfield and Paul Du Brul. (New York: Viking Press, 1977. Pp. xiv + 368. $12.50, cloth; $4.95, paper.)

Paul Du Brul served for nearly a decade on the staff of the borough president of the Bronx, New York. Jack Newfield, an editor of the *Village Voice*, is the best muckraking journalist writing today. They have joined to produce a compelling study in venality. New York City, they argue, did not "die" because of abstract processes; rather, the history of the past decade is a tale of villains and greed, a story of the "permanent government" of bankers, real estate interests, utility executives, backroom politicians, and union officials. The book is an expression of sustained outrage at their conduct in a time of collective need.

The various parts of the volume are much better than the book as a whole. Indeed, *The Abuse of Power* is more a collection of separate articles than a coherent argument about the fiscal and human plight of American cities. But some of these pieces are very good indeed. The volume contains a well-written narrative of the events leading up to the collapse of the market for New York City securities, nicely drawn portraits of Mayors Wagner, Lindsay, and Beame, and a clear presentation of the interlocked connections of the people who compose New York City's economic and political elite. All this is of no mere descriptive interest, because it challenges the inherited prevailing wisdom about the Keynesian-pluralist character of the local regime. Moreover, the deft portrait of the ways in which machine politics continues to thrive at the elite, if not the mass level in an allegedly reformed city lends credence to more analytical statements on the subject. The volume contains some useful data and information on New York City's borrowing patterns, the fiscal chicanery of Nelson Rockefeller, and on the ways in which local banks profited from the city's debt and sold their holdings at the moment of slight risk, thus compounding the possibility of default. The authors are also to be credited with an excellent description of the human side of the fiscal crisis and the severe costs entailed by budget cuts, municipal employee layoffs, and reduced services. The tone throughout these discussions, refreshingly, is one of engaged and biting anger.

With all these virtues, the volume nevertheless fails as a sustained piece of analysis. In part, its shortcomings are reflected in, and are the product of, the book's incoherent organization. Interspersed between chapters on the fiscal crisis, the "permanent" and "temporary" governments, and proposals for change are treatments of the political economy of organized crime, the arrogance of the local electric utility, the rent strike at the massive Co-op City, and the problems of municipal unions. These discussions, interesting in their own right, only have a place in the overall structure of the argument if we take the author's stated object of analysis seriously, the "death" of New York. But surely this focus is radically insufficient, even misleading. The issue is not one of "death," for New York City continues to function—in some ways, seen from the vantage point of other cities, rather well—but that of explaining the character and terms of its functioning. The term "death" serves to mask an analytical dilemma: in what ways are the desperate human and fiscal problems of New York City typical of other older large U.S. cities, and in what ways are they special? How much weight should be given to structural features of explanation, having to do principally with the place of older cities in the unevenly developing political economy of advanced capitalism, and what relative weight should be placed on individual and collective acts of self-interest and greed? One mode of analysis points to the need for major alterations in tax and incentive structures, national planning capacities, and the social control of investment; the authors' mode points in another direction, that of traditional city reform: let's clean up the mess in government so that it can act in "the public interest."

Perhaps these prescriptive modes are compatible, but an argument about their relative importance needs to be made. Surprisingly, though the thrust of much of the book points in the direction of traditional urban reform demands, the concluding chapter presents policy recommendations that focus on the economic plight of the city within a national framework: a full-employment economy, a federal urban bank, federal aid to rebuild the port, and the federalization of welfare. Only a few of the prescriptions could be implemented locally (if at all): saving rent control, and ending "waste, ripoffs, legal graft." Since so much of the volume focuses on this unholy trinity, the other proposals appear more ad hoc than they are. Disappointingly too, Newfield and Du Brul, who between them have so much personal political wisdom, born of day-to-day support for and involvement in community organizing, have almost nothing to say about the kind of politics that might in fact bring their preferred public policies into being.

IRA KATZNELSON

University of Chicago

Patterns of Metropolitan Policing. By Elinor Ostrom, Roger B. Parks, and Gordon Whitaker. (Cambridge: Ballinger Publishing Co., 1978. Pp. xxxv + 343. $18.50.)

While citizens in the 1960s complained about the quality of police services, and blue-ribbon commissions in the 1970s promulgated standards, goals, and ideals for American police, few social scientists have seriously investigated the actual organization and delivery of police services to the public. This descriptive study of police organization in 80 Standard Metropolitan Statistical Areas under 1.5 million population makes a significant step toward filling the research gap. It examines how police agencies in each SMSA are organized to provide direct services to the public such as patrol, traffic control, and crime investigation, and auxiliary services needed to produce direct services such as radio communications, pre-trial detention, police training, and crime laboratories. However, perhaps because the subject is relatively new to social science, a number of conceptual problems limit the utility of the research.

The book uses the "industry approach" to describe the varied forms of police organization. The advantage of such an approach is that it first asks *whether* the community is receiving particular services and then inquires about *which* police agency is providing them and whether there is coordination or duplication of effort among those providing police services. It is therefore possible, the authors claim, that a particular university community, for example, might receive patrol services from a campus police department, traffic control from the municipal police department, and crime investigation from a county police department. Such a consumer-oriented approach makes better sense than the simplistic earlier notions that each police agency must provide the full range of services to the people it serves. While this is a useful approach to police services delivery, the selection of services to be studied unnecessarily limits the police role in providing direct services to the public to patrol, traffic control, and crime investigation. We learn nothing about the organization and delivery of other key police services such as general peacekeeping, emergency social services, family crisis intervention, or crime prevention.

The study's basic assumption is that the organization of the delivery of police services is an important factor in determining how efficiently, effectively, and equitably police services will be rendered. This assumption receives inadequate attention, and the multitude of other factors which affect the delivery of police services is entirely overlooked. As a minimum, the political process, the legal process, and historical factors which may account for the very organizational arrangements being studied should have been explored. Purely structural factors are not likely to have the impact implicitly attributed to them in this work.

The data consist primarily of questionnaires and personal interviews to establish who provides police services in each SMSA, demographic information on the "service areas," and relevant state laws covering training standards, arrest powers, jurisdictional boundaries of authority, and so on. Problems arise, however, when non-police researchers begin asking sensitive questions of police department officials. For example, it is stated that 91 percent of the local police departments report that they receive assistance from other police agencies and 86 percent report that they provide such assistance to other police agencies. Thus, Ostrom et al. concluded that "cooperation among police agencies throughout the nation's metropolitan areas is extensive" (p. 316). Such a conclusion seems less wrong than unjustified on the basis of the evidence. Surely on the basis of newspaper accounts and personal experience, we should be cautious about accepting the police establishment's public account of interorganizational cooperation.

While the data base remains somewhat questionable, therefore, a valuable effort is made to use the data to measure several concepts such as fragmentation, duplication, and multiplicity. Such terms are often found in debates over metropolitan policing, but are rarely as explicitly defined as in this study.

While insisting that the goal is pure description, not evaluation, the authors nonetheless seem impressed with the multitude of individual patterns of organizing the delivery of police services. They conclude, "The prescription that it is necessary to organize all service activities within a single, 'full-service' police department is based more on abstract principles of bureaucratic organization than on examination of police experience" (p. 324). However, since their data basically reflect only what police departments *say* about their solutions to dealing with one another in providing services and no independent effort is made to assess the efficiency, effectiveness, or equity of these services, such a conclusion seems premature.

LEE S. WEINBERG

University of Pittsburgh

Poor People's Movements: Why They Succeed, How They Fail. By Frances Fox Piven and Richard A. Cloward. (New York: Pantheon Books, 1977. Pp. xiv + 381. $12.95.)

The power of the poor consists of their ability to generate a crisis of political instability, thereby forcing the elite to respond in ways that may be in the interest of those responsible for causing the turmoil. Members of the elite may respond repressively, and certainly are likely to be cooptive or otherwise controlling, but there are no significant alternatives for poor people otherwise lacking in political power.

This is the central thesis of Frances Piven and Richard Cloward in their influential and important book, *Poor People's Movements*. It is articulated forcefully in an introductory chapter that finds the origins of protest in the attenuation of social control accompanying occupational dislocation and transformation and mass migration, and finds elite responses, when they are forthcoming, to be primarily reactions to the disruptions, not to the demands or claims of protesters or their spokespersons. Their book will undoubtedly structure discussion of this subject for years to come.

Four case histories support the central thesis and ancillary arguments. Piven and Cloward focus on the struggles of the unemployed and of industrial workers of the 1930s, and the more recent civil rights and welfare rights movements. Each chapter rehearses the rise of insurgency and spontaneous demonstrations of defiance, the attempts of movement leaders to get astride of the movement and provide it with coherence, organization and direction, and the response of the elite both to relatively spontaneous insurgency and to subsequent organizational efforts. Cloward and Piven observe that elite members appear to have been responsive to these movements only when they threatened continued disruption of the system. Efforts to build organizational capacity, develop organizational strategies, and harness the power of the poor, they conclude, are at best irrelevant, at worst (and usually), destructive of the spontaneous rebellion that alone may result in elite concessions.

Among the many reasons to applaud Piven's and Cloward's efforts are the unity and comprehensiveness of their theory of protest and the thematic, clear treatment of the case studies, the comparative basis of their analysis, and the engaged yet critical presentation of the evidence. Of particular interest is their discussion of the welfare rights movement. The authors were initially instrumental in articulating the theoretical basis for the movement, and throughout remained involved as they watched it fall precisely into the errors against which they say they had warned.

Poor People's Movements is in large part an extended argument about the proper role of organizers and organizations in insurgent movements of the poor. Piven and Cloward assert that community organizers, labor organizers and others who would create poor people's organizations for the purpose of directing, targeting and sustaining pressure on the elite actually play the perverse role of constraining the movement's disruptive potential, thus making it easier to adopt non-responsive government programs, coopt insurgent groups, minimize programmatic concessions and, particularly, limit changes in the terms of future political relations. They urge organizers to respect the considerable restrictions of protest activity over which they have little control and to treat with humility their ability to mobilize mass dissent or influence the outcomes of elite reactions to protest. More harshly, they condemn organizers whose instincts are to build and consolidate poor people's organizations when crowds are in the streets and ready to march.

The Piven-Cloward thesis depends upon a notion of the "historical moment." There are times when poor people are prepared to rebel; these times should be exploited and not diverted into organizational activity. There are times when poor people are not prepared to rebel; these times are irrelevant and organizational activity is thus of no consequence. Unions or community organizations tend to divert energies in periods of mass rebellion, they argue. In periods of mass quiescence, they may or may not be benign, but are unlikely to be able to initiate or secure significant social change.

While Piven and Cloward make a powerful case against organizers' instincts to control and oversee organizational development, some questions may still be raised concerning the efficacy of leadership in poor people's movements. First, what is the role of organizers in periods that precede spontaneous demonstrations? How important was it that blacks in the South had at times tried to register to vote before the period of mass agitation that began around 1955? How important was it that community action agencies existed in the mid-1960s to provide support for welfare rights? Or that organizing among southern textile workers continues? If, as Piven and Cloward agree, one of the conditions of protest is poor people's belief that their cause is legitimate and supported by those in authority, then organizers may play an important part in mobilizing support for that belief, or developing leadership for future struggles.

Second, what are the organizational components of spontaneous protests? Disruption of prevailing social relations apparently can be affected by resources, skills at manipulating the media, constituents, and the target of rebellion. Piven and Cloward recognize this, but have left to others the task of spelling out the tactical considerations in organizing spontaneity.

Third, although it may be impossible to dictate the results of elite efforts to disarm disruptive protest, it is not so clear that organizers and others play no role in protest outcomes. Another chapter remains to be written on the ways in which organizers and allies of poor people's movements operate in tandem with the movements to affect protest outcomes. No doubt some prematurely cooperate with authorities to bring the crisis to closure. But if the limits of protest are as severe as Piven and Cloward suggest, then surely there is important work to be done in analyzing strategic considerations in securing most desirable or least restrictive protest results.

MICHAEL LIPSKY

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The Case-or-Controversy Provision. By James E. Radcliffe. (University Park: Pennsylvania State University Press, 1978. Pp. viii + 285. $13.50.)

Sporadic efforts by justices of the Burger Court to slam the courthouse door in the face of potential litigants has spurred the interest of political scientists in questions of access to the courts. Restrictive access rules may, as the Chief Justice believes, help solve problems of congested dockets. More fundamentally, the question of who may bring a case before the courts is important because of its direct impact on the policy-making potential of the judiciary.

James E. Radcliffe's contribution to this growing literature focuses directly on the most basic source of access doctrine, Article III, Section 2 of the Constitution, the import of which is that federal judicial power may be exercised only in a "case or controversy." In this book Radcliffe presents an exhaustive (and sometimes exhausting) analysis of the Supreme Court's interpretation of this clause, finding considerable support for his major hypothesis that the Court has expanded federal judicial power by gradually relaxing the case-or-controversy requirement.

While this conclusion is unimpeachable, questions remain about how much Radcliffe has added to our understanding of developing access requirements. Particularly unsatisfying is his effort to separate the "essential elements" of the case-or-controversy clause from judicially created access doctrines. Derived essentially from the majority opinion in *Muskrat* v. *U.S.*, 219 U.S. 346 (1911), the essential elements of a case or controversy include adversity, a sufficient interest in a legal right, "an actual justiciable controversy," and a case on which a court can render a final, binding judgment. Radcliffe analyzes the Court's decisions bearing on each of these elements in painstaking detail, including separate chapters on sources of legal rights and remedies and the development of the notion of sufficient interest. Often Radcliffe seems to be beating a dead horse as he recounts the details of case after case in which the justices touched on one or another of these elements of a case or controversy.

The author is at his best when he writes (more briefly) about such judicially created doctrines as standing, ripeness, mootness, and political questions. Under close examination these doctrines and not the "essential elements" of *Muskrat*, emerge as the basis for most recent Supreme Court decision on litigant access. Moreover, Radcliffe shows that each of these doctrines has its roots in the case-or-controversy clause. In the last analysis the distinction between essential components of the case-and-controversy provision and the judicially created doctrines is entirely artificial.

The book is also weakened by Radcliffe's failure to deal with some of the more significant questions political scientists might ask about access policy making. For example, we need to investigate how the voting decisions of individual justices on access questions relate to their positions on substantive policy issues. Radcliffe's focus on what "the Court" has decided largely blinds him to differences between the justices and forecloses consideration of the less legalistic aspects of his topic.

In short, this book is not as interesting or as significant as it might have been. Yet, because of its generally thorough and impressively documented treatment of an important constitutional provision, it will provide valuable background material for future researchers concerned with access to the courts.

EDWARD V. HECK

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Cancer Crusade: The Story of the National Cancer Act of 1971. By Richard A. Rettig. (Princeton, N.J.: Princeton University Press, 1977. Pp. xix + 382. $15.00.)

As this book's subtitle indicates, its central purpose is to tell a story. If this had been its only purpose, the book would have been reasonably successful in achieving its goal. But because the author of *Cancer Crusade* purports to do more, his work must be evaluated in broader terms. Richard Rettig sets out not only to write a descriptive legislative history of the 1971 Cancer Act, but also to shed new theoretical light upon the politics of agenda-setting and policy implementation, particularly the former. He also seeks to write a book that cuts across and communicates effectively to three distinct audiences—concerned biomedical personnel and policy makers, the general public, and political and social scientists. The first two audiences can be reached by the good description the author provides by carefully examining public hearings, documents, personal interviews and journalistic accounts of the National Cancer Act. The third audience, the political and social science community, is more demanding when evaluating the merit of policy case studies, judging them in terms of their theoretical insights and implications for future public policy research. On this score Rettig's book fares far less well.

The first third of *Cancer Crusade* details the pre-legislative agenda setting activities that, Rettig argues, largely predetermined the legislative outcome of his case. These early chapters are long on description and short on theoretical reflection. Despite the author's citation of major literature on agenda-setting, almost no actual use is made of the right insights contained in this literature. The dialogue at times lapses into a presentation of chatty anecdotes of dubious analytical relevance. Rettig uses many direct quotations from speeches made by various cancer crusaders. Readers are left to fend for themselves in sorting out influential from non-influential speeches, substance from symbol. One useful insight drawn from the author's description of agenda-setting is that the mind-set of policy makers often is strongly influenced by successful "role models" of past programs when deliberating the feasibility of new policies, regardless of the appropriateness of these models. Thus, for instance, much use was made of the space program "model" by policy elites weighing a new "categorical" and highly management-oriented NASA type approach to cancer research and control, despite considerable scientific controversy concerning the basic causes of cancer.

The second part of Rettig's book consists of nearly 150 pages describing the legislative history of the National Cancer Act. The author's treatment of legislative politics is interesting enough, although not especially novel or arresting. He relies too heavily upon the legislative hearings that provide the structure and much of the content of the chapters in this part. (For example, over 90 percent of the footnotes in chapter 6, "Two Days in March," are drawn directly from hearings held by Senator Kennedy's health subcommittee of the Senate Committee on Labor and Public Welfare, presented virtually in the same order of pagination as found in the original hearings.) In fairness, other parts of the book are more original in structure and content than this. Little more can be said about this part of the book except to note its thoroughness of detail.

Only the concluding chapter that comprises the third part of *Cancer Crusade* seriously attempts to transcend factual description and offer analytical generalizations. In this chapter Rettig seeks to relate his case study to the broader theoretical literature on agenda-setting, policy making and implementation, and policy impact. His analysis of agenda-setting is, in my view, flawed at the outset by his insistence that the policy initiators of the cancer crusade were "a unique constellation of [wealthy] private citizens grouped around Mary Lasker," rather than an "organized interest group" (p. 282). This curious terminological distinction leads him to conclude that the cancer initiative was a rare case of wealthy individuals spearheading a policy, with manifest support from organized labor, and latent support from public opinion. Yet wealthy corporate elites quite often have been able to mobilize such support for numerous policies that were sold as beneficial to the "community-as-a-whole" (e.g., the initial support for the now-discredited urban renewal policy; general support for federally subsidized research and development policies of all kinds; even the space program itself). The remainder of Rettig's analysis of agenda-setting tells us little more than that agendas are set in stages of development and that a "push factor" is needed to transform general beliefs into specific policy proposals. Rettig's attempt to shed new light on policy making and implementation is likewise lacking in the non-obvious. Here we find such truisms as: presidential support is a useful political resource, and wealthy people know how to use power and have easy access to political decision makers. Despite these shortcomings, it is nice to find here yet another confirmation of the fact that technical policy analysis often serves mainly symbolic functions. The appearance of policy analysis often is more important than the content of analysis in influencing a policy's reception by political elites.

Rettig's discussion of the policy impact of the National Cancer Act of 1971, while brief, is the most useful part of his book. He offers a thorough assessment and critique of the actual consequences of national cancer policy. We learn, for example, that attempts to improve the diagnosis, prevention, and treatment of cancer have been impeded by program leaders' decisions to support only rehabilitation research, to apply only existing knowledge to cancer control, and to exclude delivery of patient care from its definition of "control," thereby limiting organizational efforts to demonstration, field testing, and dissemination projects. More importantly, even if they had attempted to do more, they would have been subject to heavy criticism, since even the best existing diagnosis and treatment techniques are medically controversial. Thus, the more effort agency elites made, the more the agency would become subject to charges of waste.

In one area, that of carcinogenic environmental chemicals, vigorous agency action might well have prevented much cancer, even in the absence of major research breakthroughs isolating new underlying disease mechanisms. In this area we learn from Rettig that the National Cancer Institute avoids the political struggle necessary to obtain the withdrawal of known carcinogenic chemicals from use in the economy. Rettig also clearly demonstrates that rapid increases in funding for cancer research and control have come at the direct expense of funding for most other medical research, particularly basic biomedical research. What is more, this social cost of the policy is a direct outgrowth of a budgetary bypass provision of the original legislation, that enabled the National Cancer Institute to submit its budget requests to the OMB, unchecked by other possible NIH or HEW policy priorities.

When engaged in impact analysis, Rettig is at his best. One wishes that more than 20 pages of *Cancer Crusade* could have been devoted to this approach. It would have enabled him to reach the third audience that has largely eluded him.

MICHAEL P. SMITH

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Progressive Cities: The Commission Government Movement in America, 1901–1920. By Bradley Robert Rice. (Austin: University of Texas Press, 1977. Pp. xix + 160. $10.95.)

This volume is not a study of the operation of cities under the commission form of local government but rather a description of the "origin, diffusion and decline of the commission plan" (p. xvii). The author portrays how the plan was developed by a group of businessmen in Galveston, Texas at the turn of the century and how it spread throughout the country due to the tremendous publicity given to it (p. 110). However, Rice fails to prove or disprove the Hays-Weinstein thesis that the commission plan was an upper-class struggle to wrest control of city hall from the lower class.

Rice correctly observes that the commission movement never reached Scott's institutional stage in the theory of diffusion of municipal innovation. Although some state legislatures passed laws permitting municipalities to govern themselves as they wished, there was never any encouragement to conform to the "Texas Idea" (p. 19). As a matter of fact, early supporters like Richard Childs later switched their support to the council-manager system.

The commission plan's failure to achieve wide popularity cannot be attributed entirely to the successful challenge of the council-manager plan (p. 111). If only five percent of American cities are operating under the commission form today, this is because of a number of weaknesses of the plan itself. The first is the concentration of policy making and administration in the hands of the Board of City Commissioners. Progressive era reformers sought to divide the two for more efficient and economical government. Although current theory no longer believes that the two must be distinguished, many students of public administration feel that it is still necessary to separate them, especially in personnel management, to preserve the merit principle.

The second weakness is the amateurish quality of management under the plan. Each commissioner supervises one or more administrative departments. The only qualification these commissioners have is having been elected. Accordingly, there have been instances in which a plumber was elected to run a police department, a barber to manage an engineering department, and an undertaker to direct a health department. Once assigned as supervisor to an administrative department, a commissioner resents interference, and to preserve harmony, colleagues avoid criticizing each other. As critics of the plan put it, the Board of City Commissioners is transformed into a "fraternity of tolerance," an automobile with an accelerator without a brake, five city mayors instead of one, each virtually a "petty potentate." As a result, deadlocks and delays occur in the conduct of city management.

The third weakness is the role conflict among the five commissioners. One of the commissioners is elected president of the board and is popularly known as the city mayor. Under the plan, the mayor is just another commissioner; in practice, however, he plays a very complex role due to pressures from community residents who look upon him as a political leader or as their delegate who must respond to their demands. Thus, the mayor's perception of his role in municipal government is more than that of a presiding officer at city commission meetings or as representative of the city at ceremonial functions. He sees it as a focal point of leadership in local public administration, community politics and intergovernmental relations. In so doing he could step upon the toes of his colleagues, rock the boat and make waves in city hall. On the other hand, his colleagues in the commission perceive his role as a coequal or just another commissioner whose duties, responsibilities, and salary are no more or less than theirs.

The book suffers from numerous typographical errors. To cite just a few, plan was misspelled as *play* (p. 3), yards as *yeards* (p. 16), trial as *trail* (p. 20), capital as *capial* (p. 27), dissipated as *disapated* (p. 28), presence as *presense* (p. 49), concerned as *conccerned* (p. 58), and municipalities as *municialities* (p. 59). In spite of these deficiencies, Rice's reliance on primary historical data and his objective presentation of the material makes this book worthwhile. His narrative on the rise and fall of the plan is consistent with good and sound historical scholarship. On the whole, this piece of research enriches our understanding of the diffusion process in municipal innovation and broadens our knowledge of a dying breed of local government structure.

A. B. VILLANUEVA

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The Democratic Party Primary in Virginia: Tantamount to Election No Longer. By Larry Sabato. (Charlottesville: University Press of Virginia, 1977. Pp. ix + 169. $9.95.)

This small volume, published for the Institute of Government at the University of Virginia, follows in the tradition of Virginia electoral studies established by the late Ralph Eisenberg. It is a thoroughly researched history of the primary election as a nominating system in Virginia from its inception in the early part of the century to the present, backed up with extensive, simply presented, tabular data on its use both on a statewide and district (congressional and state general assembly) basis, and interspersed with some modest theoretical conclusions and comparisons of Virginia primary laws with the legal prescriptions for primaries in other states.

For some time now historical revisionists have pointed up the ironies of progressive electoral reform, citing particularly the effect of "popular" nominating processes in reducing the party organization's role in interest aggregation, issue mediation, and vertical integration of the electorate. The South's use of the primary—as a surrogate for party competition, a foundation for white supremacy, and a bulwark against centralization—was a further extension of this disaggregative effect and a major contributor to the bourbonization of southern politics. Virginia's experience with the primary illustrates the substantial diversity of this fragmentary political institution in relation to the historical and cultural variation within the states of a South which has been represented for most of the twentieth century as a stereotypical political monolith. And some further ironies are revealed in the uniqueness of Virginia's political history when viewed from the rather confining perspective of the state's nominating practices.

Virginia was the last southern state to adopt the primary as a major feature of its nominating processes. The Democratic Convention of 1904 approved a primary plan for nomination to offices elected on a state-wide basis in anticipation of the gubernatorial election of 1905, but it was not until 1912 that a state primary law was enacted. Even then the primary requirement in Virginia was less comprehensive than it was in the other southern states, and that method of nomination continued to be optional in most of the contests. Only in Democratic gubernatorial nominations has it been consistently used. Unlike the other states of the South, Virginia did not provide for a second, or run-off gubernatorial primary until 1952. Even then the second primary was held only upon request of the candidate in second place in the first primary, and in 1970 the legal provision for a run-off was dropped.

The limited use of the primary in Virginia as compared with the other southern states can be attributed almost entirely to the fact that the oligarchic domination of the state that began with the Martin organization in the 1890s and continued with the Byrd machine through the mid-1960s had largely accomplished the consolidation of its means of self-perpetuation (the most limited electorate in the country and a comprehensive local factional organization) by the time the primary began to be extensively used for that purpose elsewhere. Because of these historical circumstances, two general judgments of the author (as well as the subtitle of the book) seem somewhat incongruous, or at best less applicable to Virginia than to some other southern states. The first is that "the primary was the vehicle for political control of the Old Dominion during most of the twentieth century" (p. 120). The second is that a few southern states "had some of the characteristics of a bifactional system, but none to the degree possessed by Virginia" (p. 91, n. 6). The problem here is the use of the term "bifactional." Virginia was really *uni*factional, with only token opposition from sporadic sources, whereas in Louisiana, for example, the Long and anti-Long factions were substantially competitive for more than 30 years.

The book is marred by inadequate editing not only in substantive matters but also in preparation of copy. One might cite numerous examples of syntactical awkwardness, but a single metaphor stands out as a redeeming Freudian (or perhaps intentional) slip. After having professed a bias only in favor of a two-party system, the author refers to "the Republican-infested Sixth and Seventh [congressional] districts."

In keeping with the purpose of the study, an experientially based prescriptive conclusion is offered: the nominating method for both parties that might best comport with the dual criteria of party responsibility and popular participation would be some variation on the pre-primary convention, which would either endorse candidates for the primary or open the possibility for a challenge primary. At a time when institutional and policy analyses are attempting to fill the theoretical vacuum that has become such an obvious feature of the discipline, a comprehensive historical study of this type provides some suggestive points of departure for the development of a theory of elections, not only in relation to a single unit of analysis but for comparative purposes as well.

WILLIAM C. HAVARD

Vanderbilt University

Blue-Collar Soldiers? Unionization and the U.S. Military. Edited by Alan Ned Sabrosky. (Philadelphia: Foreign Policy Research Institute, 1977. Pp. ix + 166. $5.95, paper.)

Recent changes in military compensation and organization, coupled with general social trends toward industrial democracy, have generated an interest in the potential for unionization of the American armed forces. In particular, the linking of military compensation to civil service salaries has put the largest federal civil service union, the American Federation of Government Employees (A.F.G.E.), in the position of fighting for higher military compensation in the course of trying to increase the salaries of its civilian members. In the fall of 1976, the A.F.G.E. amended its constitution to allow itself to recruit, as members, active duty military personnel, so that it might someday collect dues from those who were benefiting from its activities. Largely in response to this event, a number of studies and conferences dealing with military unionization were initiated in 1976 and 1977.

Blue-Collar Soldiers is based upon one such conference, organized by the Foreign Policy Research Institute. It is an unusual book in that it combines dispassionate analysis with extremely impassioned advocacy papers both for and against military unions. The proceedings of any conference are likely to be of uneven quality, and this book is no exception. As an editor, however, Sabrosky has done a better than average job of integrating the contributions.

The two best papers in the book are by leading scholars in the field of military unionization. William Taylor, a permanent member of the social science faculty at West Point, has outlined the basic issues involved in the potential unionization of the American military. Although he is too prone to see the problem of unionization tied to the conversion to an all-volunteer force, his presentation is one of the most balanced introductions to the field available.

Ezra Krendel, of the Wharton School, presents an excellent overview of the status of military unions in Western Europe. The European experience is frequently cited as the most useful precedent for consideration of military unionization in America. Krendel does much to dispel the belief that unionized armed forces are ineffective as combat organizations. I believe that he tends to overestimate the similarities between the European and American situations, although he does indicate an awareness of historical differences.

The advocacy chapters are qualitatively different from the scholarly analyses. David Denholm of the Public Service Research Council, and Theodore Humes, a Staff Counsel for the Senate Judiciary Committee, present a case against military unionization (and public sector unionization generally), based largely on a 1976 public opinion survey that contained three questions on military unions. A large majority of the sample (82 percent) is reported to be opposed to military unionization. An equal percentage is reported to be opposed to military strikes. The tabular presentation of the data shows no respondents undecided on the issue, an outcome I have never seen in survey data on a controversial issue. What the tables do suggest is that support for unionization (and for strikes) is highest among those social groups most likely to serve in the military: the young, those with low levels of education, and nonwhites.

In response to a question of whether or not there should be a union shop in the event of unionization, the authors report that "an unusually high number refused to answer the question because they were absolutely opposed to military unionism. . . ." According to the table, 9 percent of the respondents failed to answer this question, and there are no data presented on their motivations.

As *Blue-Collar Soldiers* went to press, the A.F.G.E. was polling its locals to determine whether it should in fact enroll military personnel. In the interim, the locals have voted against such enrollment, the Defense Department has issued a directive against unionization, and, at this writing, a strong statutory prohibition has been passed by the United States Senate (S-274) and is likely to pass the House of Representatives. The likelihood of unionization of the American military would appear to be remote. What remains to be addressed would appear to be an underlying malaise in the military personnel system, of which the unionization issue is but a symptom.

DAVID R. SEGAL

University of Maryland

At the Grass Roots in the Garden State: Reform and Regular Democrats in New Jersey. By Vicki Granet Semel. (Cranbury, N.J.: Fairleigh Dickinson University Press, 1978. Pp. 287. $18.50.)

In a short note, "In Praise of Style," Philip A. Schrodt laments that " 'professional' writing in the social sciences is almost invariably cited by journalists and other writers as the deepest abyss into which the use of the English language by educated people can sink." In ironic apology for his own writing, he adds: "I have, in accordance with 'professional' standards, eliminated all 'colloquialism,' maximized the use of jargon, and in general attempted to keep the prose as dry and uninteresting as is humanly possible" (*PS* 11:352).

At the Grass Roots in the Garden State, based upon Vicki Semel's Ph.D. dissertation, could be nominated as the archtypical example of social science writing of the sort Schrodt describes. Laboriously overwritten, chock-full of jargon, juiceless as a mouthful of saltines—the prose is so painful to read that it vitiates the impact of what is otherwise a respectable piece of scholarship. The tell-tale feature of this book is that no editor can be found listed among the author's many acknowledgments. A shame—her publisher did her wrong.

Beneath the infelicitous prose, however, lies an interesting thesis. The author suggests that although the reform Democrats of the late 60s and early 70s shared many of the traits of previous reformers, they nonetheless represented a new breed of party activist. In contrast to the populists and the early civil service reformers, they were neither anti-urban nor anti-industrial. In contrast to the progressives, they sought not to weaken the political party as an organization but to capture it. In contrast to the Stevenson Democrats, they attempted not merely to democratize the party's internal procedures, but also to direct the organization toward securing substantive liberal policies. And unlike the stereotypic "amateur Democrats," these new reformers were willing to expend the time and effort required to perform such mundane tasks as registering voters and getting them to the polls on election day.

The primary evidence adduced to support this thesis derives from some 633 responses to a mail questionnaire sent to 1000 delegates who attended the founding convention of the New Jersey New Democratic Coalition (NDC) in March 1969 and from an additional 770 responses received from a sample of approximately 2300 Democratic County Committee members (precinct leaders) stratified to match the NDC delegates by sex and by county. The questionnaires, mailed out in the spring and summer months of 1970, contained a number of questions used previously by Eldersveld (*Political Parties,* Chicago: Rand-McNally, 1964) and Marvick and Nixon ("Recruitment Contrasts," in Marvick [ed.], *Political Decision-Makers,* Glencoe, Ill.: Free Press, 1961). This enables Semel to compare and contrast the attitudes and self-described behaviors of regular and reform Democrats of New Jersey with one another and with the attitudes and self-described behaviors of party activists surveyed in the 1950s.

Semel's initial expectations are formalized as 15 major hypotheses and many sub-hypotheses. These are tested for substantive significance largely by using Goodman and Kruskal's gamma to see how strongly the relationships are ordered in the expected direction. The initial expectations that characterize NDC members as Stevenson or "amateur" Democrats with middle-class backgrounds and liberal attitudes are generally confirmed. But surprises soon emerge: the NDC activists show greater concern than expected for substantive as opposed to procedural (internal party democracy) issues; they show unexpected willingness to perform election-oriented tasks; and they are both interested and successful in seeking higher party office.

There are a few minor errors of interpretation (e.g., in Table 21, p. 159, two variables which correlate at −.01 are described as "sustain[ing] each other"), and an apparent contradiction between hypothesis 8 ("no significant difference between reformers and regulars in purposive motivation") and hypothesis 8d ("Reformers will emphasize significantly more single-minded purposive motivations," p. 145), but by and large the data are interpreted carefully—with appropriate caution if not stylistic grace.

On the basis of her findings Semel suggests that movements like the NDC could serve genuinely to reform and reinvigorate local party organizations. Far from being destroyed, as some critics have alleged, traditional organizations could be transformed. Policy concerns could displace personal material rewards as the major incentives for party activism, and cosmopolitan (national and international) concerns could displace local ones as the major foci of attention.

Given these speculations, the omission of developments subsequent to the data collection is unfortunate. Did the NDC remain a significant force in New Jersey politics? What reforms, if any, have occurred within the Democratic party as a result of the NDC's activities in the early 70s? A chapter on these developments not only would have given the book greater currency, but it would also have served as a rough test of the author's thesis. Updating the extensive bibliography beyond 1972 would also have made this book more valuable as a general reference on party organization.

As it stands, *At the Grass Roots in the Garden State* seems useful mostly for those with a special interest in local party organization, but it offers little of interest for others, even for those who are just curious about party politics in New Jersey.

MICHAEL MARGOLIS

University of Pittsburgh

Economic Policy Beyond the Headlines. By George P. Schultz and Kenneth W. Dam. (New York: W. W. Norton, 1977. Pp. xi + 225. $8.95.)

Most of this book, written by two academics who served as high-level economic policy makers from 1969 to 1974, deals with the important specifics of the national budget, tax policy, wage and price controls, income security, international economic policy, energy policy, and governmental reorganization. The chapters on the mechanics of the budget cycle and on tax policy are particularly valuable. Shultz and Dam are not good at proposing reforms; in fact, their general policy advice is often, "If in doubt, do nothing." The book is a reflective, informal, and often shrewd account of six years' experience of cabinet-level economic policy. Although less technical, it is roughly the 1970s equivalent of Arthur Okun's report on the 1960s, *The Political Economy of Prosperity.* Students of political economy and economic and social policy should read the book.

The authors' approach to policy exemplifies the apolitical and often antipolitical ideology of the *economist administrator.* The economisting elements in this doctrine include an emphasis on the putative efficiency achieved by relatively unregulated markets, a preoccupation with the costs of intervention by noneconomic actors (like courts and environmentalists) into economic transactions, and a plea for more thought about the long-run consequences of economic policy—a plea surely endorsed by nearly all academic economists who have ever gone to Washington. The administrative side of the doctrine includes the implicit belief that the public interest is better served by cabinet members of the executive branch than by others, an emphasis on central coordination, a benign view of the power of the president, and a stress on the negative features of Congress (too many delays, too much influenced by special interests, too little rational analysis), of the courts (too interventionist), and of the bureaucracy (too balkanized, too much influenced by its clients). In the name of efficiency, practitioners of administrative economisting seek to fend off both the deserving equity appeals and the appallingly greedy special interest ploys interjected into economic policy making by politicians, pressure groups, and, indeed, voters.

Of course there are alternative views. *Economic Policy Beyond the Headlines* should be read along with Louis Fisher's excellent study, *Presidential Spending Power,* which presents a quite different model and, further, documents the often illegal and gimmicky actions of the executive that thwart the constitutional budgetary powers of Congress. Debalkanization of the bureaucracy and greater central coordination have well-known potential costs, and, even under the present administrative arrangements, presidents have corruptly exploited their power to control the economy—as in the 1972 re-election campaign. The book is sketchy with regard to many of the political aspects of economic policy during the Nixon years; greater, although still insufficient, detail is found in Safire's *Before the Fall.*

Economic Policy Beyond the Headlines opens with a short, general essay on policy making. Written by two people who have actually made policy, the analysis is particularly refreshing compared to the abstracted scientism of professionalized public policy studies and, more importantly, provides one of the superior answers to the question, "What did you learn during those years in Washington?"

EDWARD R. TUFTE

Yale University

Policy Analysis and Deductive Reasoning. Edited by Gordon Tullock and Richard E. Wagner. (Lexington, Mass.: D.C. Heath, 1978. Pp. xii + 201. $18.00.)

This collection provides an essay on the relative advantages of deductive approaches to policy analysis by John Blair and Stephen Maser and 11 examples of such analysis. Topics treated include criminal behavior, hospital cost control, several issues in taxation, social security, affirmative action, bureaucratic behavior, public investment discount rates and government purchasing. Three make exceptionally innovative contributions to their areas of inquiry: Miner and Chalace on tax reform and legislative behavior, Mackay and Weaver on bureaucratic behavior, and Shepsle on discount rates. All but one of the articles (Urken on purchasing) are useful analyses as well as illustrations of deductive approaches to policy analysis.

The uses of deductive analysis are varied and the analyses in the volume reflect that diversity. Miner and Chalace provide an excellent example of a short, direct derivation of unobvious and counter-intuitive hypotheses from widely accepted assumptions. Examples of logical examinations of problems to select critical elements for further analysis are found in articles by Weaver on social security, Worcester on affirmative action, Wagner on the tax treatment of philanthropy, Aranson on criminal sentencing, and Brennan on indexing income taxes to inflation. The reconciliation of different conclusions from apparently similar models is the focus of Shepsle's analysis of public investment discount rates, and Austin and Gordon extend a conclusion from one body of work into a new area in their analysis of hospital cost control. In their analysis of criminal justice, Orr and Ramm analyze the difficulty of treating abstract concepts such as liberty and equality in a deductive framework. These applications illustrate that deductive approaches are not a mechanical cranking through of simple assumptions to derive a conclusion. The problem areas are much too complex for simple applications, and the usefulness of an analysis depends heavily on the analysts' knowledge of the subject matter as well as their ability to follow logical implications to a conclusion.

While the articles are good contributions, they also illustrate common problems with deductive approaches. Some draw strong policy conclusions while neglecting an alternative interpretation which would emerge from modification of a single empirically testable assumption. A good example of this is Brennan's conclusion that income tax indexing to inflation is not important, a conclusion which depends on an assumption of median-voter voting outcomes. The very next article by Mackay and Weaver argues that median-voter voting outcomes are unlikely in most institutional settings—especially in those dominated by decision making in Congress and in congressional committees. If the Mackay-Weaver conclusion had been used as an assumption in Brennan's analysis, Brennan would have reached a different conclusion. Analysts must be careful about drawing strong conclusions from deductive approaches in complex settings. Such conclusions are really hypotheses which themselves should be empirically testable and viewed with caution until verified by additional evidence.

While the volume includes a variety of analyses, not all types of deductive analysis are included. There are no examples of the deductive derivation of hypotheses followed by empirical testing nor examples of the identification of critical assumptions which produce different conclusions followed by empirical examination of the alternative assumptions to determine which has the most evidence for its support. Hence, two examples of the most fruitful applications of deductive methods are missing. Overall, however, the analyses are well done, provide variety in approach and subject, and illustrate that deductive approaches, even if they do not include empirical testing, may be very useful.

ROBERT L. BISH

University of Maryland

The Memoirs of Chief Justice Earl Warren. By Earl Warren. (Garden City, N.Y.: Doubleday, 1977. Pp. xii + 394. $12.95.)

This is a pleasant book, but I suspect that it will disappoint legal scholars. There simply is not much disclosed in *The Memoirs of Chief Justice Earl Warren* that has not already been revealed. There are several good unauthorized biographies of Warren, and although they are more journalistic than academic in import, they have done a competent job in portraying the chief justice's life. Still, *Memoirs* is worth reading and reviewing. After all, Warren ranks with Marshall and Hughes as one of our most distinguished chief justices. And this is his story, his perspective of a lifetime dedicated to public service.

Warren began to work on his *Memoirs* in 1970. He had just about completed the first draft when he died in 1974 of a cardiac arrest. The editors engaged in a certain amount of "editorial tinkering" prior to publication, but the book is far from polished in its final form. It is discursive and suffers from a lack of detail where detail is demanded (especially in the chapter dealing with the Supreme Court years). Still, it must be remembered that the rough draft was written while Warren was in his eighties and undoubtedly would have been improved had he lived to refine it.

The book is organized chronologically. Each chapter deals with a life passage: boyhood remembrances, his crime-fighting years as a district attorney, an interlude as attorney general of California, his political role as governor, campaigning for the vice-presidency, and the Supreme Court years. The final two chapters deal with Warren's perspective and assessment of his political critics (particularly the A.B.A.) and his role as chairman of the president's commission charged with investigating the assassination of President Kennedy.

While there is not much new to be gleaned from *Memoirs,* it does provide a number (though far too few) of "behind-the-scenes revelations" (editor's phrase). For instance, one scenario describes a chance meeting between Warren and Eisenhower in which Eisenhower chastized Warren for his "soft" approach to the "Communist cases," in particular *Yates* v. *United States* (p. 5). Ironically, the *Yates* opinion was written by Mr. Justice Harlan, a conservative Republican judge appointed and esteemed by the president. In another episode involving Eisenhower, Warren describes how he was invited to a White House dinner while the school desegregation cases were under consideration. The president also had invited John W. Davis, counsel for the segregation states, and spent much of the evening praising Davis. After dinner, Eisenhower took Warren aside and said, "These are not bad people. All they are concerned about is to see that their sweet little girls are not required to sit in school alongside some big overgrown Negroes" (p. 291). Warren goes on to narrate an even more blatant attempt to "surreptitiously influence the action" of the Court (p. 337). This time the culprit was Attorney General John Mitchell. Mitchell dispatched an emissary from the Department of Justice to meet with Justice Brennan, and later Warren, in an effort to provide the Court with some wiretap information on foreign embassies which had not been included in the record of a case pending before the Court. In addition to anecdotes such as these, Warren does go into some detail about his resignation from the A.B.A., a subject which he had refused to discuss prior to the publication of the *Memoirs.*

Another contribution of the *Memoirs* relates to the character of Earl Warren. From this personal narrative one can perceive the qualities of candor and strength imbued in Warren's personality which contributed to his successful guidance of the Court for a period of 16 years. This persuasive ability was manifest throughout his public life. And while scholars may be critical of the chief justice with regard to his "task leadership," he more than made up for this deficiency with his mastery of interpersonal relations.

One of the continuing focal points of discussion which has concerned public law scholars relates to the question of whether Warren experienced a change in philosophy after he was elevated to the High Bench. It is Warren's contention that no change occurred. The chief justice writes that "my actions have been exposed to the public constantly for more than half a century, and I feel that my views and actions in later years are but an outgrowth of the earlier ones" (p. 5). In support of this, the *Memoirs* highlights a wealth of progressive accomplishments achieved while district attorney, attorney general, and governor of California. Indeed, Warren constantly comments on the influence of Hiram Johnson upon his thinking and actions. While this theme of "progressive consistency" is persuasive, it emerges largely because little attention is given to incidents in Warren's career which would portray a contrary picture.

The conservative image of Warren as a tough law enforcement officer while serving as district attorney and attorney general does not emerge from *Memoirs.* Rather, he comes across as firm, but fair and equitable—a role with which libertarians can feel quite comfortable. But there is insufficient information in the book to determine which of these contrasting roles is correct. Perhaps the best way to resolve this "consistency" puzzle is simply to acknowledge that constituencies change, and with the change in constituencies, perceptions of problems and the solutions thereto also change. Warren does discuss at some length the factors that led to his support of the Japanese relocation while he was attorney general (a decision which he later came to regret). But no attention is given to his controversial support of the compulsory flag salute requirement of school children in California or a number of non-libertarian Court decisions such as *Irvine* v. *California.* Students of the Court anxious for enlightenment on these facets of the chief justice's life will be unfulfilled.

The major disappointment with *Memoirs* is that little attention is devoted to the years in which Warren served as chief justice. The chapter pertaining to 16 of the Court's most important and controversial years is but 41 pages long. There is a good fund of information pertaining to the *School Desegregation Decisions* in the chapter, but the attention given to other cases is such that little of consequence is gained. Even the reapportionment decisions which Warren deemed to be one of the hallmarks of the Warren Court's contributions are discussed in the briefest fashion. Equally disconcerting is the paucity of information regarding the chief justice's relationship and interaction with his fellow justices. Public Law scholars who had hoped to gain intimate glimpses of the "behind-the-scenes" revelations during the Supreme Court years are sure to be disappointed.

CLIFFORD M. LYTLE

University of Arizona

Governance of Federal Regulatory Agencies. By David M. Welborn. (Knoxville: University of Tennessee Press, 1977. Pp. viii + 179. $9.50.)

In the literature on American politics, independent regulatory commissions have never been very much admired as government organizations. Their independence of executive control has raised questions about their accountability to the public, and has led to the constant charge that they have sold out to the industries they are supposed to regulate. Their status as organizations headed by a collegial executive has also been held against them in a culture generally disposed to accept the Hamiltonian proposition that governmental efficiency requires single rather than plural leadership.

Now come complaints from all sides that commissions interfere with the effective functioning of the economy in the sectors they regulate. It is argued that commission policies keep prices higher and production lower than would be the case if such economic decisions were left to the competitive play of a free market. Deregulation is the theme of the day; with the inexhaustible optimism characteristic of reformers, those calling for deregulation claim benefits every bit as extravagant as those once made for regulation itself.

Onto this scene David Welborn's book on regulatory commissions makes a timely entrance. It examines how regulatory commissions make decisions, and it presents findings that may surprise as well as inform students of the regulatory process.

One of the book's most attractive features is that—unlike much of the literature on this subject—it has a substantial empirical base. Welborn conducted 66 interviews with present and former chairpersons, commissioners, and staff members of regulatory organizations. Seven regulatory agencies are included within the scope of his inquiry: the Civil Aeronautics Board, the Federal Communications Commission, the Federal Maritime Commission, the Federal Power Commission, the Federal Trade Commission, the Interstate Commerce Commission, and the Securities and Exchange Commission.

Welborn's central analytical concern is who "may be said to regulate and on what basis?" He focuses his research on the interaction between the commission chairpersons and both their fellow commissioners and the members of the various agencies' staff. Welborn's conclusions plainly contradict the view of such recent critics of the regulatory process as the Ash Council during the Nixon era. Welborn finds that the chairs of these commissions do in fact exercise very substantial control over their operations. This is generally true of all seven of the agencies he studied, though somewhat less pronounced in the case of the FCC and the ICC. As Welborn puts it:

> The domain of chairmen is extensive. They have substantial influence on critical aspects of staff activity. At the commission level, they have considerable opportunity for independent action and considerable influence in collective decision-making. Their capacity to affect regulatory values and objectives—the basic stakes in agency government—is much greater than others in the commissions (p. 131).

All in all, Welborn argues, the authority of a commission chair is not significantly less than that of a single executive presiding over an ordinary executive agency. The commissions, he writes, "are not true plural executive systems" (p. 132).

The deference of commission members to the chair is quite remarkable. The only real constraint they impose on the chairperson's position of leadership is the requirement that they be informed before steps of any significance are undertaken. Welborn thinks several factors are responsible for this willingness of commission members to accept the chair's leadership. Perhaps the most interesting of these factors is the commissioners' fear of the growth of the power of the staff and their belief that a strong chair can preserve the influence of the commission over agency decision making. Better in their eyes to have a strong chair than a bureaucratized agency.

In addition to demonstrating that regulatory agencies are not as pluralistic as is sometimes believed, Welborn also shows that they are far from being as independent as their statutory position might suggest. By and large the chairs of these regulatory organizations have been willing to coordinate agency decisions with White House policies, and the strength of chairpersons has thus served to facilitate presidential influence over the decisions of supposedly independent regulatory commissions.

The book is not without flaws. It is not always well written. And it can be argued that the generally favorable view the book presents of commission performance reflects the fact that it is largely based on interviews with commission members, past and present. But overall it is an impressive piece of work, greatly enriching our understanding of the internal life of regulatory bodies.

FRANCIS E. ROURKE

Johns Hopkins University

The Investigators: Managing F.B.I. and Narcotics Agents. By James Q. Wilson. (New York: Basic Books, 1978. Pp. xi + 228. $10.00.)

In view of how much is said today, and with such confidence, on the subject of bureaucracy, there is a surprising dearth of studies of individual agencies in actual operation trying to carry on the tasks for which they were created. *The Investigators* by James Q. Wilson, helps to fill that gap. It is a study of the Federal Bureau of Investigation and the Drug Enforcement Administration—two of the national agencies chiefly charged with law enforcement in the United States.

The theme which animates and structures this book is Wilson's belief that the law enforcement tasks these agencies were established to carry out have had very little impact on the way in which they are managed. In his words: "The administrative systems of the Federal Bureau of Investigation and the Drug Enforcement Administration have been shaped by the political maintenance needs of the two organizations rather than by the nature of the tasks the organizations' operators performed" (p. 192).

Thus, Wilson finds that neither organization has done much to develop or enhance the indispensable skills their employees need to carry on their law enforcement role effectively. The critical task of the operating personnel in both the FBI and the DEA is investigation. FBI agents spend most of their time gathering information on cases where a violation of federal law is suspected. Thus the essential skill an FBI agent requires is an ability to undertake a productive interview. DEA agents, on the other hand, focus their energy on trying to cultivate sources of information on drug traffic.

While this "informant development is, for DEA agents, a skill as important as interviewing methods for FBI agents," Wilson finds "little evidence that either organization attempts systematically to discover why some have the skill or to develop that skill in others" (pp. 50–51). He notes that the training programs of both agencies make only a weak effort to improve the interviewing or informant development techniques that it is essential for their agents to have.

Why do these law enforcement agencies devote so little attention to enhancing the ability of their agents to do their jobs? Because, Wilson argues, the executives in these organizations give the highest priority to warding off threats to their agency from external critics or competitors. Some of the most interesting material in the book is the discussion of the way in which executives in each of these agencies seek to cope with the political hazards in their environment.

However, while the point of departure of the book is the conviction that the best way to reform or reorganize any agency is to focus on the tasks it is trying to perform, Wilson concedes at the end of his analysis that it would be a sad day for a free society "if a powerful law enforcement organization were free to design and operate its law enforcement system solely, or chiefly, on the basis of what the agents' task required" (p. 215).

The book thus exhibits a kind of puzzling ambivalence in its attitudes toward public administration. On the one hand, it argues that efforts to improve the performance of any executive organization should be chiefly concerned with its ability to carry out the tasks that are its assigned goals. Public agencies should in other words be operated like private organizations. But in the end Wilson seems to back away from this conclusion and to look with apparent approval at the fact that these law enforcement agencies have, in his view, been highly responsive to public opinion—changing the nature of the tasks they undertake in accord with shifts in public opinion.

One subject the book avoids grappling with is the degree to which national law enforcement in the United States has in recent history been shaped neither by the imperatives of the task nor by the pressures of public opinion but rather by the vagaries of the strong personalities in charge of the major police organizations. J. Edgar Hoover at the FBI and Harry Anslinger at the head of the national narcotics agency provide vivid demonstrations of how much the direction of an organization's activity may be shaped by the personal predispositions or idiosyncrasies of its chief executive. But the impact of executive personality on administrative activity has not been easy for organization theory to handle. It is a very messy factor—definitely not suitable for elegant paradigms.

The book has many fine qualities. It is, for one thing, extremely well written and very interesting, characteristics which—unhappily—set it apart from a great many other books written in the field. It will stimulate and provoke thought, since—as is Wilson's wont—it leans against the negative slant of much of academic writing on law enforcement organizations. And, notwithstanding this perspective, the book is fair. Wilson's attitude toward law enforcement officials in the agencies he studies is sympathetic but certainly not uncritical.

FRANCIS E. ROURKE

Johns Hopkins University

Power in the City: Decision Making in San Francisco. By Frederick M. Wirt. (Berkeley: University of California Press, Institute of Governmental Studies, 1978. Pp. xi + 417. $6.95, paper.)

This book, originally published in 1974, is now available in paper, thanks to the efforts of the author and the University of California Press. Frederick Wirt's delineation of the politics of San Francisco is embedded in an unusually broad portrayal of historical politics and of trends and forces in population, social relationships, the economy, corporate structure and action, and other levels of government, as well as local governmental institutions. Thus the book has earned high praise both from sociologists and political scientists. It is called "a masterpiece, lucid and compelling . . . good social history as well as good sociology" (*Contemporary Sociology* 5:356–57), and Lester Salamon's review (*Public Administration Review* 36:418–29) is a theoretical contribution as well as an enlightening perspective on this book.

Wirt develops a fruitful treatment of historical decision making in three key areas—economic development and redevelopment, ethnic and racial group demand and governmental response, and the politics of elections, governance, and reform. In each, coalition formation and structure, interaction, and key outcomes are described. With Wirt's broad view of the events and actors, the method provides an entirely sensible antidote to the sometimes ahistorical and too narrowly governmental blinders of political scientists.

Wirt found a fragmented, "hyperpluralist" political system in San Francisco, mediating group demands and only intermittently organized by temporary coalitions for particular goals, and structured by a city charter that effectively insulated the levers of city government from mass political demand. When corporate interests wanted to remake downtown San Francisco with high-rise office buildings served by a regional rail system, they were able to forge an economic-development coalition with labor unions and politicians and to harness governmental powers partly outside the charger (the BART District and the Redevelopment Agency, created under authority of the state). Lesser classes had to struggle with a system that placed one institutional obstacle after another between their political aspirations and control over programs and allocations. Really well-organized popular groups (well, the unions, anyhow) could sometimes secure pieces of the pie, but cooptation rather than the political incorporation of broader group interests remained the common result. This pluralistic system, it should be noted, operates within a constitutional structure that still preserves the victory of business over working-class interests effected by the reform of 1898, when a popular (millionaire) mayor was able to split the opposition of the labor movement with the same tactics of deference and cooptation that help to keep the system fragmented now.

The basic structures of control and initiative that Wirt described as of the early 1970s are still alive, if not entirely well. Events finally overtake the best of descriptions, of course. Aside from modest but perceptible shifts in the recent success rates of various interests, some significant changes partly underscore Wirt's account. The passage of Proposition 13 proves again Wirt's argument as to the city's vulnerability to external constraints, even though San Francisco's strength in Sacramento has so far staved off the predicted dire effects. Potentially just as significant, still tentative coalition efforts among white and some minority groups engineered the passage of district elections for the Board of Supervisors in 1976, a return to the system in use before 1898. It is possible that this victory will eventually be seen as a key step toward lasting coalition and toward the dismantling of the reform charter. The confrontation of demands for fiscal constraint and for social reform may yet produce coalitions of the sort notable by their absence in this century.

Teachers of urban politics should consider using this book. Although its length calls for selective use, much of the writing is gracefully interesting. Images of realities and possibilities tantalize the perceptions. To the untutored student, the questions that Wirt holds deliberately open might be felt as frustrating ambiguity, and the richness of detail might be experienced as an entangling thicket, rather than as opportunity to exercise one's own skills as observer and theorist. Hence one efficacious use might be to have the instructor pose controversial theoretical arguments (e.g., pluralist versus neo-Marxist), then turn students loose on the book to test key propositions and to work out other hypotheses that lurk in the discussion and findings.

As one who has also struggled to understand San Francisco, I continue to appreciate the validity of Wirt's findings and to learn from the network of relationships set forth. Although the book is not a statement of theory in a formal sense, Wirt offers proposals for the reorientation of our perspectives on community power, and the theoretically oriented reader will find much thought-provoking material here.

RUFUS P. BROWNING

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Comparative Politics

China's Industrial Revolution: Politics, Planning, and Management, 1949 to the Present. By Stephen Andors. (New York: Pantheon Books, 1977. Pp. xviii + 344. $17.95.)

What I like most about this book is the focus on the intersection of politics and economics. What I like least are editorial statements about, for example, the "progressive weakening of capitalism as a civilization" or that "we in the West tend to ignore China's revolution or reject it." The author addresses important questions, but mixes chaff with wheat in his answers.

Andors is at his best describing industrial planning and development. He is at his worst when he attempts to compare Marxist and Western visions of modernization in an introduction which adds little to the subject—unless, that is, one agrees with the author's effort to elevate Frederick Taylor (of time/motion fame) to a status equivalent with Hobbes and Weber.

The study deserves praise because Andors documents the fusion of thought and practice in China between politics and economics. The Chinese have, for some time, been aware of the virtues of worker participation and control in and over industrial decisions; results include increased worker efficiency, higher quality output, and increased personal efficacy.

The work addresses the evolving relationship between industrial planning, management and worker participation from 1949 to the early 1970s. The author's description of how the Chinese grappled with questions of planning and efficiency, by using various centralized/decentralized schemes accompanied by changes in incentive systems, is more useful than the efforts by Pekinologists to discuss the same underlying events: the period of post-revolution reconstruction, the Great Leap Forward and consolidation in its aftermath, the Cultural Revolution, and recent events. By concentrating on the political organization of the industrialization process, particularly worker-management authority relationships, the author illuminates a key area of the Chinese Revolution. The Chinese—like any other modernizing people, have faced, on the one hand, the issues of urbanization, population growth and economic development, and, on the other hand, the need to create and sustain a new set of political institutions to cope with these changes. However, eschewing centralized "storming" strategies used by the Russians, the Chinese have come to realize that an efficient strategy to achieve their goals is to unite their production operations with their participatory system of worker-management; through revolutionary committees workers manage and managers work. The author argues, convincingly, that this strong emphasis on participation in plants, communes, etc., has a great deal to do with the substantial economic development success the Chinese have enjoyed.

I think it fair to say that without the Marxist perspective, this book would not have been written. Too few political scientists yet grasp the essential connection between politics and economics. My quarrel is not with the Marxist approach but with the specific nature of the assumptions laid down by the author, i.e., his interpretation of the Marxist versus capitalist views of modernization and the Chinese addition to Marxism. Marxist and liberal economists do make different assumptions about human nature, although the self-interested rational-choice individual of liberal economists is, in principle, congruent with the Marxist emphasis on class-based explanations of economic and social phenomena. The real issue is over holistic vs. methodological individualism explanations. For example, one could account for the Chinese pattern of worker-management control in terms of standard economies and diseconomies of scale ideas from the theory of the firm. The Chinese simply experimented with combinations of incentives and sanctions in worker-management relations until they reached a reasonable mix given their level of technological development. Schurman's explanation of the role of ideology—on the "red" over expert idea—provides a basis for a plausible interpretation of the pattern of industrial development described by Andors as well. When the existing value system has been eroded—as was the case in pre-Communist China—it is important for political elites to generate an explicit set of symbols through which communication can occur and new values can be internalized. Not only does the emphasis on participation in industry help the Chinese establish a Marxist egalitarian alternative to the previous hierarchical Confucian code, but it also makes eminent sense in an economic system which is at a low technology state where detailed centralized coordination of plants is not appropriate and self-reliant industrial and agricultural enterprises minimize imports and maximize growth.

However, Marxism is an exciting alternative research program, one which provides structuring principles with which to look at development problems. One hopes that "normal" political science on China is stimulated by this work.

ROGER W. BENJAMIN

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Women in Russia. Edited by Dorothy Atkinson, Alexander Dallin, and Gail Warshofsky Lapidus. (Stanford, Calif.: Stanford University Press, 1977. Pp. xiii + 410. $18.75.)

Women in Russia is the product of a conference on women in Russia held at Stanford University, May–June 1975. The book represents a judicious selection of papers from the many given at the conference, chosen, according to the editors, primarily for their topical coherence. Given the perennial problems associated with multi-authored books, this one is particularly successful in maintaining continuity of thought and relevance of content between chapters. This success may in large measure be attributed to the high quality of the individual contributions, written by some of the foremost scholars in sovietology and/or women's studies. In its caliber of research, the book sets a standard of excellence.

Women in Russia follows a procession of post-World War II studies on women in the Soviet Union, starting with Norton Dodge's pioneering *Women in the Soviet Economy* (1966), going through *The Role and Status of Women in the Soviet Union,* edited by Donald R. Brown (1968), the result of the first American conference on women in the USSR held at Bryn Mawr College, and ending up with three publications in 1977, of which this is the last. It would almost seem as if there were a surfeit of volumes on the subject of Soviet women. Yet the fascination of the topic continues unabated, as several new contributions came out in 1978, my *Women under Communism* among them.

U.S. researchers are apparently more interested in the status of Soviet women than in the position of women in virtually any other foreign nation, including the People's Republic of China. The Soviet Union was the first country publicly to endorse a program of women's liberation. Indeed, the Bolsheviks came to power promising much in this area. We in the West are curious about the progress of the Soviet experiment. For one thing, we would like to learn from the Soviet experience in order better to shape the women's movement in our own society. For another, we want an objective assessment of the Soviet claim that only under socialism are women truly liberated. And perhaps we half hope and half fear that authoritarian power is more capable of coping with the complex process of attitudinal change than an open society.

Women in Russia provides us with some of the latest research into the success and failures of the Soviet party and government's policies towards women. The ongoing record, as the authors present it, reveals just that: success and failure. Part 1 includes three papers on the Russian heritage, among which Alfred Meyer's "Marxism and the Women's Movement" deserves special mention. Part 2 deals with sex roles and social change. Beatrice Farnsworth's study of the 1926 Marriage Law debate provides intriguing insights into the traditional attitudes of the Communist party membership of the time, while the essays by Ethel Dunn, Michael Sacks and Janet Chapman carefully document the economic progress of Soviet women. Part 3 focuses on women in Soviet society and politics and covers such topics as those discussed in Mollie Rosenhan's "Images of Male and Female in Children's Readers," Jerry Hough's essay on women's issues in Soviet policy debates, and Colette Shulman's "The Individual and the Collective," perhaps the most original contribution in its attempt to investigate Soviet attitudes.

The book essentially confirms what is already known: (1) The family remains the core unit of Soviet society and is likely to remain so indefinitely. (2) Women, for whatever reasons, continue to be discriminated against in the economic, social and political life of the USSR. (3) Soviet authorities have tried to implement a policy of increased social services to reduce discrimination and to lighten the burden of "the double shift" with still modest results. (4) Economic priorities continue to take precedence over social priorities and women are caught in between. (5) Women retain traditional attitudes regarding the woman's role, although work outside the home has been accepted by most as not only necessary but desirable in itself.

The value of the book may be said to lie in its conscientious updating of previous data, not in any new outlook on women's problems in the Soviet Union nor in any new answers to the concerns powed above. With few exceptions, the topics follow those covered in earlier research. The editors appear to be aware of shortcomings, citing the deliberate omission of cultural and literary aspects of the subject, and, in my opinion, the more serious omission of any consideration of the demographic problem. However, since it is manifestly impossible to cover every aspect of a subject in a volume of this length, this type of self-criticism would not seem really warranted.

My own criticism of the otherwise excellent book is its emphasis on description rather than analysis, and its failure to provide an analytic framework in which to examine the subject. In the effort to achieve objectivity, the editors could be accused of a certain timidity. The vitality of the findings given at the original Stanford conference have in large part been worn away by editing that leaves the facts but forces readers to cull out their own explanations. We are reminded that Western preferences should not intrude on Soviet reality, that the Soviet Union has had enormous problems to solve in the course of its history, only one of which has been women's liberation, and finally, that the failures of the leadership in this area might well be the failures of any leadership. In fact, the Soviet government has done a great deal to improve women's lot and the ultimate criterion should be whether Soviet women are happy or not. These sorts of reminders would seem unlikely to appear in studies devoted to racial or religious discrimination, and indeed are of dubious validity. The question is persistent: Why, after 60 years of Soviet power, power officially dedicated to changing women's status, is the Soviet woman still tied to traditional roles and discriminated against in her effort to advance economically and politically?

The question is central to the general consideration of the position of women in modern society. The study of public policy towards women in the Soviet Union opens up new areas of investigation into the problem of sexual discrimination: the relation between personal codes of conduct and ideological and political rigidity; the relation between attitudinal change and state paternalism; and the relation between the achievement of group self-consciousness and political development. This book, like many of its predecessors, remains bound to the specifics of the Soviet *qua* communist situation. The question of women's assumption of an equal place in society demands broader perspective. The Soviet experience can tell us a great deal about the role of ideology in providing a conceptual vehicle for a specific type of developmental change. But ideologies are limited by time and space. The record of women's liberation in the Soviet Union is in one sense the record of the success and failure of one ideology in this area. The leadership has carried the process as far as its Marxist-Leninist vision can take it, given its commitment to Piroges' "industrial paradigm" and traditional values. Similarly, the Western record is the product of the Western emphasis on political rights. The problem is how to adapt ideology grafted onto tradition to a changing environment, how to learn to live in Kariel's open-ended world without sacrificing identity. An analysis of the situation of women in the Soviet Union from a perspective such as this might well provide a more productive framework for investigation than the dead-end family-work, capitalist-communist models so far assumed.

BARBARA JANCAR

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On the Dictatorship of the Proletariat. By Etienne Balibar. (London: New Left Books, and Atlantic Highlands, N.J.: Humanities Press, 1977. Pp. 237. $13.00.)

There are three reasons why a reviewer would welcome Graham Locke's translation of Etienne Balibar's *On the Dictatorship of the Proletariat.* It is of topical interest: Balibar's discussion of the twenty-second congress of the French Communist party illuminates the historic status of "Eurocommunism." It is, moreover, a significant contribution to the theoretical discussions within Marxist-Leninism concerning the state. Lenin's *The State and Revolution* is a crucial text in this canonical reconstruction of the theory of the state, and one that Balibar subjects to a detailed reading. Balibar's book is of final interest to current preoccupations within political science about the state and ideology.

Balibar's book, on one level, marks an extension of his discussion of the state and historical transformations in his *Cinq Etudes sur Materialisme Historique* (Paris: Maspéro, pp. 65–102). But Balibar's book can also be read as an entirely immediate response to the twenty-second party congress. During the pre-congress debate, Georges Marchais suggested omitting the concept of the dictatorship of the proletariat from party statutes. This concept was considered "anachronistic" and fundamentally incompatible with the real interests of French Communists, namely a democratic transition to socialism. Balibar aptly describes the theoretical and political lessons derived from the Congress: Marxist concepts can't be dogmatically conceived and then extrapolated from "lines of argument and proof which underlie them, from becoming blanket solutions, formal ready-made answers to every question" (p. 37).

Balibar issues one further caveat. Marxist concepts such as the dictatorship of the proletariat cannot be conceived in their relation to bourgeois ideology. Balibar is at his most persuasive in analyzing the framework in which the decision to drop the concept of the dictatorship of the proletariat is rooted. For the twenty-second party congress posed the debate within a deceptively simple alternative. Either one abandons the concept of the dictatorship of the proletariat or one abandons the democratic transition to socialism. The congress made use of three oppositions to illustrate these divergent paths. The first is the distinction between peaceful and violent means. The democratic road to socialism excludes civil war, police repression, white terror and red counter-terror. Secondly, there is a parallel distinction between legal and illegal means. The democratic road to socialism would "allow the existing system of law to regulate its transformation without recourse to illegality" (p. 39). Finally, one distinguishes between majoritarianism and a minority party. The dictatorship of the proletariat conceives of the working class as a minority exercising power. Advocates of the peaceful transition ask: doesn't the socialist state now represent the democratic power of a majority? Moreover, the doctrine of majority will guarantee the possibility of a peaceful transition to socialism. Yet these seemingly innocuous pairs of alternatives undermine the Marxist theory of historical materialism, supplanting it with a model of political "choice."

Balibar next demonstrates how these alternatives are in turn linked to three false ideas: (1) the equation of the dictatorship of the proletariat with the history of the USSR; (2) the definition of the dictatorship of the proletariat as only a particular political regime; (3) its definition as the point of transition to socialism. Balibar gives three cogent counter-arguments (which are elaborated in Chapters 3, 4 and 5). For, might not the history of the USSR represent a process and a tendency in contradiction with the dictatorship of the proletariat? Secondly, conceiving of the dictatorshp of the proletariat as a particular configuration of institutions implies that the choice between different paths of transition is fundamentally a choice between *systems* of institutions: parliamentary (pluralist) against nonparliamentary (single party): "Revolutionary Marxism is here subordinated to the abstract categories of bourgeois 'political science' " (p. 47) applied *a posteriori* to the field of class struggle and its history. Lastly, the dictatorship of the proletariat can only be defined as a point of transition to *communism.* "This shade of meaning changes everything" (p. 49). For, Balibar convincingly argues, to define the dictatorship of the proletariat in terms of socialism is to remain within the bourgeois ideological framework.

Balibar consistently demonstrates the importance of the theoretical framework in which the concept of class struggle is worked out; for "the theoretical misunderstanding of the class struggle is not simply a theoretical event: its result is that the proletariat can lose the practical initiative bought at a high price, it can become the pawn of social relations of exploitation and oppression instead of the force capable of transforming them" (p. 55).

Graham Locke's translation is scrupulous; his introduction is pointed and informative. One should note that this edition differs from the French edition which includes two dossiers—one of texts from the twenty-second party congress and one of basic texts of Marxist theory. This translation includes all documents pertaining to the twenty-second party congress. The English translation also includes a lucid afterword by Balibar as well as an essay by Louis Althusser not included in the original French edition.

DIANE RUBENSTEIN

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Angola Under the Portuguese: The Myth and the Reality. By Gerald J. Bender. (Berkeley and Los Angeles: University of California Press, 1978. Pp. xxvii + 287. $15.00.)

This study critically reviews the history of European settlement and race relations in Angola. A product of a decade of research and writing and a revision of a doctoral dissertation, Gerald Bender's book presents an overview of Portuguese policy and practice during five centuries of colonial rule. Bender's analysis, while not surprising, constitutes a devastating attack and exposé of the myth of Portuguese egalitarianism and harmonious racial mixing in Africa.

The first part treats "Lusotropicalism," an ephemeral and mystical ideology used by the Salazar and Caetano regimes before 1974 to explain and justify Portugal's presence in Africa. Bender identifies the origins of this ideology in Brazilian and Portuguese writings and thought, policies and legislation. Then he traces the Portuguese effort to validate lusotropical claims of racial assimilation and democracy through the example of Brazil, and he contrasts racial patterns in Brazil and Angola. Bender demonstrates that the Portuguese dream of Angola becoming the "Brazil of Africa" was shattered by actual experience. He shows that miscegenation was not a reflection of national qualities such as Portuguese nationality. Further, the model of Brazilian race relations did not match the economic, political, and social inequalities between whites and nonwhites in Brazil as well as Angola.

A second part discusses white settlement in Angola, looking at numbers and background, economic activities, settlement patterns, and racial attitudes during the colonial period. Two experiences are analyzed in considerable detail. One was the use of *degradados* or exiled criminals who for the most part were the white settlers of Angola. Bender traces the history of this penal colonization and concludes: "It is a supreme irony and tragedy that the instruments employed by Portugal to carry out its civilizing mission had been drawn, until the past fifty years, from the dregs of Europe's most underdeveloped nation and were themselves considered beyond the pale of civilization" (p. 94). This practice of drawing whites from the lowest elements in Portuguese society resulted in a class of emigrants who lacked educational and occupational skills. The second experience was that of white settlement in rural Angola. The Portuguese believed that white settlement of the hinterland would assure them sovereignty in the colony. Here Bender's field research yields invaluable data and analysis. Despite government propaganda to the contrary, only a small number of Portuguese rural peasants actually responded to the call for settlement in Angola's planned communities; most who did so soon abandoned the settlements, while those who stayed depended on African workers rather than themselves. Land conceded to whites brought a catastrophic impact on Africans in some areas. The large sums of money for the projects exceeded those for public education; yet by 1969 only 840 Europeans or one percent of the European population in Angola were successfully integrated into the settlement schemes. Even government investments in black settler schemes did not result in increased African productivity.

Part 3 of the book looks at colonial practices from slavery to the war of independence, examining the old policies of the new state, revealing practices of forced and contract labor, and, finally, identifying the Portuguese wartime responses to Africans. After 1963 the Portuguese generals advocated resettlement of Africans as the key to victory. The dislocation of the African population resulted in loss of income, inability to maintain subsistence, depletion of soils in intensively occupied areas, lower level of nutrition, higher frequency of disease and epidemics, greater social conflict, and discontent.

A concluding part offers a personal assessment of all these consequences. Bender's indictment of Portuguese policy and practice is drawn along perceptive and informative lines. His study stands as a serious contribution to an understanding of race relations in Angola. With emphasis on a critique of the Portuguese experience, data and analysis are tightly synthesized, yet clearly and cogently presented.

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Yundong: Mass Campaigns in Chinese Communist Leadership. By Gordon Bennett. (Berkeley, Calif.: Center for Chinese Studies China Research Monographs, 1976. Pp. 133. $4.50, paper.)

Revolution at Work: Mobilization Campaigns in China. By Charles P. Cell. (New York: Academic Press, 1977. Pp. xxi + 221. $14.95.)

Although the key role played by mass mobilization campaigns in post-1949 Chinese politics has long been acknowledged, there have been, aside from occasional case studies of a single campaign, surprisingly few attempts by scholars to examine systematically either the functions which these mass campaigns perform or to assess more generally the success or failure of the campaign strategy as a whole. This makes the two books under review especially welcome. They overlap in their emphasis on the value of the campaign strategy for China's economic development and socialist transformation, and in their laudable desire to go beyond a discussion of only the most well-known campaigns; however, there are important differences between them, stemming in part from their dissimilar methodologies and the audiences toward which they are directed.

Gordon Bennett's monograph is addressed both to beginning students and to specialists on contemporary China, a not inconsiderable challenge which he handles surprisingly well. His study attempts to marshal data in support of three broad conclusions. First, campaigns offer Chinese citizens an effective vehicle for political participation. Second, on balance they contribute more to economic growth than they take away. Third, they will continue to thrive even after the present generation of leaders has passed from the scene. Although one may take issue with one or more of Bennett's conclusions, the real value of his work does not depend on one's agreement with his findings, which he recognizes as tentative, but rather resides in the *tour d'horizon* we are given of campaigns in China. Separate chapters treating the origins and development of the campaign style, the process of organizing and running a campaign, the multi-task function of campaigns, the debates—both in China and the West—over the value of the campaign strategy, and mass compliance and non-compliance during campaigns provide a wealth of information which makes the monograph a very useful supplementary taxt.

Charles P. Cell's book, on the other hand, is a more specialized account, being a revision of his doctoral thesis. Whereas Bennett was content to test his hypotheses on the basis of qualitative data, Cell elects to "move beyond the documentary library research approach into field investigation and the use of quantitative data." Unfortunately, while one sympathizes with Cell's methodological aspirations, his study is ultimately less convincing than Bennett's more modest enterprise. Briefly, Cell has chosen 36 campaigns and subdivided them into three categories: economic, ideological (aimed at correcting erroneous thoughts, raising consciousness, etc.), and struggle (aimed at resolving antagonistic contradictions). Using four major types of sources—interviews with informants who had personal knowledge of campaigns, personal experience based on a five-week trip to China, primary source materials such as newspapers and policy documents, and secondary source materials—Cell is able to generate indicators of mobilization, achievements and shortcomings of campaigns. Then, using Guttman scaling, Cell ranks the campaigns in terms of his three variables: level of mobilization, achievements and shortcomings. Finally, the variables are correlated to ascertain the extent of campaign achievements and shortcomings produced by each category of campaign at given levels of mobilization.

Cell finds, among other things, that (1) "the campaign system as a whole is a utilitarian method for China's goal of socialist transformation" and (by no means the same thing) that "without the campaigns China would . . . be considerably further away from socialist transformation"; (2) struggle campaigns, although producing lower achievements and having greater shortcomings than other types of campaigns, may be the 'most important in the overall process of China's transformation into a fully socialist society"; (3) "economic campaigns at given levels of mobilization show greater levels of achievements and lower levels of shortcomings compared with other types of campaigns; (4) "ideological campaigns produce the lowest level of achievements as well as shortcomings, although their concurrent levels of mobilization are equally low" (p. 172).

Any pioneering effort is likely to have problems; to Cell's credit, he is aware of at least some of them. For example, one key to constructing a successful scalogram is the selection of items that might be likely to form such a scale. In this respect, there are problems with

Cell's choice of indicators. Are the indicators he has chosen to measure achievements and shortcomings really reliable? While one could readily agree, for example, that "leftist or rightist deviations of policy" or "violations in the implementation of policies" are clearly shortcomings, one would be hard pressed to discover many mass campaigns without such problems. Yet, because his scalogram of necessity has a threshold level, we find that a campaign like the Training of Revolutionary Successors of 1964 contained both of the above shortcomings, the 1954–55 Campaign to Eliminate Counterrevolutionaries contained the second shortcoming but not the first, and the Urban Communes Campaign of 1958–59 contained neither. Apart from the fact that the majority of his refugee informants were former students who had been through the first campaign and thus were more sensitive to its shortcomings (particularly if they were of bad class origin), there is the more general problem here relating both to the vagueness of his indicators and the problems of coding. Many of his indicators, particularly those measuring achievements (e.g., "raise political consciousness"; "new skills learned") and shortcomings are simply too subject to varying interpretation. Moreover, because his indicators are geared to measuring only the more immediate, short-term effects of a campaign, Cell's scales yield some rather strange results. Can we really conclude, for example, that because the aforementioned Urban Communes Campaign scaled high on achievements and low on shortcomings it was necessarily a success? I think not.

Another problem, which Cell himself raises in a paragraph near the end of his book, stems from his decision to weight each campaign equally. It seems rather foolhardy to place the 1954 Campaign to Study the Constitution on the same level as the Great Proletarian Cultural Revolution (GPCR) or the Great Leap Forward in a study of campaigns in China. Achievements scored in many such minor campaigns may be much less important than the shortcomings of one campaign like the GPCR, as witnessed by the comments some in China have recently made to visitors about the "ten wasted years" prior to the overthrow of the "gang of four" in 1976. If one accepts this latter criticism, the value of Cell's entire enterprise, whatever his findings, seems questionable. Actually, it is when Cell departs from his quantitative analysis, for example in his discussion in chapter 2 of the interrelationship of campaigns over the 1949–1975 period, that he is at his most convincing.

One final difference between Cell and Bennett concerns their assessment of the future of mass campaigns in China. Treating the campaign strategy as essentially a part of the "Maoist vision," Cell, unlike Bennett, concludes on a rather pessimistic note regarding the future use of mass campaigns in China. Bennett, stressing such attributes as the range of tasks performed by campaigns, the revolutionary political culture which has taken root in China, and the continuing support for campaigns among certain sectors of the elite, is more sanguine about the long-range possibilities for the survival of these campaigns.

STANLEY ROSEN

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Facing Up to Modernity: Excursions in Society, Politics, and Religion. By Peter L. Berger. (New York: Basic Books, 1977. Pp. xix + 233. $11.50.)

This book is an intellectual disappointment to anyone who remembers *The Social Construction of Reality* or Berger's contributions to the sociology of religion. It is even a disappointment to those of us who may have rejected much of the analysis in *Pyramids of Sacrifice* but found that it at least tackled the large moral questions of Third World development. The problems are twofold. First, it is essentially a collection of articles on such diverse topics as marriage, revolution, religion, psychoanalysis, the New Left, murder, patriotism, class, ethnicity, foreign policy, and more. As such, it suffers from a lack of intellectual cohesion which the title and a brief introductory essay fail to repair. Second, the articles themselves were written for such diverse audiences that the level of analysis oscillates wildly from piece to piece—ranging, for example, from a thoughtful sociological analysis of marriage to petulant and ill-thought-out broadsides on American foreign policy and "the socialist myth."

For political scientists, the two most interesting pieces in the volume will undoubtedly be "Reflections on Patriotism" and "In Praise of Particularity." Together, they constitute elements of a credo that Berger obviously hopes might spark a revitalized conservative movement in the United States. Berger's basic thesis is that it is from what Durkheim calls "the little aggregations" of neighborhood, family, church, and ethnic group that human identity and meaning are derived. These aggregations are precisely those most threatened by the huge dominating institutions of the state, corporations, and labor unions. Thus, Berger concludes, our major task is to foster the development of such small mediating institutions which are the only buffers between a hegemonic state on one side and the atomized individual citizen on the other. Liberals who for at least the last 45 years in the U.S. have believed that a beneficent government should deal exclusively in universalisms are thereby unfit by ideological predisposition to undertake such a task. Conservatives, he allows, are not much better equipped, for while they see the alienations of big government, they fail to see the "alienations of big business" and its homogenizing market.

From the fact that the "mediating structures" he champions are not on either major party's agenda Berger draws the comforting conclusion that the issue is at least nonpartisan. There is surely much to be said for the case that small-scale solidarities not only reduce anomie but also provide the essential basis for such minimal forms of self-determination as are left to local publics. But readers of almost any political persuasion will be disquieted with some of the conclusions which Berger draws. First, it is not just a question of the survival of "mediating structures," but also a plea that "public policy should utilize the mediating structures as its agents" (p. 138). One wonders what would happen to the autonomy and adaptability of such small aggregations if they were to be made the lowest rung of administration. Berger considers this problem in passing and concludes, "I have no definitely reassuring answer to this question, and it troubles me" (p. 141). Refreshing as it may be to have Berger raise this major issue, one might have thought it would detain him for more than a single paragraph. A second difficulty is that Berger sees a warrant in his position for a whole series of policies which would probably undo more of the liberal dispensation than many conservatives would welcome. He would have the state end its efforts to get minimal standards for school or residential integration. He favors a political system in which the privileged life styles of upper classes (mediating structures?) will be spared undue incursions from the unwashed. He favors, along with many democratic theorists, a "low participation" political system both because "full participation" conduces to fanaticism and, in any event, would be a nightmare of endless meetings. Berger's construction of "full-participation" is fairly representative of the "straw-manning" many of his arguments require.

The basic substructure of the collection seems to be a single idea: we essentially have a choice between a fairly tolerable, if somewhat unjust, political reality *and* the likelihood of human chaos if we follow the pathological, coldly rational, linear, utopian, schemes of the Left. This is hardly a new idea, and Berger has little new to add to Edmund Burke. And in conveying these ideas Berger permits himself many polemical characterizations in the place of serious analysis. Thus the left is pictured as uncritically admiring Eastern European socialism. Communist China comes in for some of Berger's least restrained polemics. He calls Maoism "a moral monstrosity," a "holocaust" founded on "hecatombs of murdered human beings" (p. 112). In the context of a larger argument which calls for "moral costs/benefits analysis," one might have expected at least a brief analysis of the human costs of the *ancien regime* which Maoism has replaced. Does the Chinese peasantry experience the current regime as chaos compared to the "order" of pre-1948? The point is not that I fail to share Berger's political prejudices (although I do fail to share them) but that they remain at the level of prejudices with little or no serious analysis. One would expect far more from a sociologist from whom one had learned so much in the past.

JAMES C. SCOTT

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Contemporary Venezuela and Its Role in International Affairs. Edited by Robert D. Bond. (New York: New York University Press, for the Council on Foreign Relations, 1977. Pp. x + 267. $15.00, cloth; $6.95, paper.)

This readable collection of seven essays was published as a Council on Foreign Relations book. According to a brief explanatory note, while "not commissoned" by the council, the essays were subjected to "a critical review . . . by a Council study group." Not surprisingly, these essays fall within what could be described as a liberal establishment perspective.

The book's editor, Robert D. Bond, a research associate with the council, acknowledged the common ideological stance of the essays in the introduction: "Needless to say, our perspective on Venezuela is not universally shared. There is, from the left, a strong critique . . . which often argues that the leaders have betrayed their own promises, institutionalized capitalism . . . contributed to the marginalization of the poor . . . and squandered oil wealth. . . . The corresponding critique from the right charges the leadership with having sacrificed economic rationality to socialist ideology, discouraged much-needed foreign investment, [and] incompetently managed petroleum affairs" (p. 3).

Tugwell's essay on U.S.-Venezuelan relations is replete with evidence of the generally inadequate United States response to Venezuelan initiatives and concerns. Despite the fact that Venezuela has been a steady and reliable supplier of oil to the United States in time of war and Arab embargo, we have persistently refused to grant it a "hemispheric preference." In fact, the U.S. Congress "punished" Venezuela and Ecuador for their membership in OPEC. "This was done partially because of the misconception (quite common) that it was OPEC, rather than OAPEC (Arab Petroleum Exporting Countries), that embargoed the United States" (pp. 209–10). Tugwell's second essay in the volume on the oil nationalization could very well be used as a learning manual by all resource-rich Third World nations considering expropriations.

Similar to Tugwell's, Kim Fuad's delightful essay on Venezuela's role in OPEC suggests that the United States may have missed the boat when it failed to respond positively to a 1959 Venezuelan initiative that would have had an international commission govern the supply and price of oil. This Venezuelan proposal would have included the consumer nations along with Venezuela's fellow producers in the international governance of this commodity. While new "light" oil reserves should soon be discovered in Venezuela, Fuad feels that Venezuela's future as a major oil power will depend on its being able to exploit commercially the huge reserves of "heavy oil" found in its Orinoco region.

John Martz' essay on Venezuelan relations with its Caribbean and Latin American neighbors attempts to relate domestic factors to foreign policy. Venezuela, according to Martz, has a handicap in relating to its neighbors. On one hand, it is a "democratic island in a sea of authoritarian regimes" (p. 194). On the other hand, its poorer neighbors in the Caribbean and in Central America perceive its initiatives to be that of a new-rich "petroimperialist" nation out to recolonize them.

The two background essays on Venezuelan politics and economic development by Daniel Levine and Pedro Pablo Kuczynski adequately complement the above-mentioned four essays. Levine's essay focuses on the survival capacity of democracy. He suggests that the "political arrangements" made in the 1960s between the democratic reform-minded political elites and the economic, social and military elites may not operate as smoothly in the 1980s as it has over the past generation. He cites the weakening of the organizational capabilities of the political parties and the concomitant depoliticalization of the masses to support this concern. Compromise should also become harder to achieve in the future since the revenue from oil is expected to decline in the 1980s as more expensive off-shore "light" oil and the Orinoco "heavy" oil is necessarily exploited.

Kuczynski's essay similarly expresses concern about Venezuela's economic development strategy. Its industrialization options are limited by its relatively high wage rates and hard currency. Concentrating on resource-based capital intensive industries would, however, lead to an increased marginalization of the rural and urban poor. The recent rise in the rate of inflation and urban unemployment may be an early warning sign of a new political crisis.

While this collection of essays admirably fulfills its stated purpose, the picture they present of Venezuela's political system appears to be overly simplified. Middle-level politicians, public sector managers, business people and military officers are presented as stereotypes. In addition, a number of crucial assertions could have been better documented.

I accept the underlying assumption of the book that scholars should not be overly critical of the caution shown by Venezuela's democratic leadership. The 1973 Chilean tragedy demonstrated the need for "reform mongerers" to move with caution in Latin America. Venezuela has emerged as one of the articulate advocates for Third World concerns. Whether or not it can maintain this new international role will depend largely on how well it keeps its domestic political and economic house in order.

DAVID EUGENE BLANK

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Political Culture and Political Change in Communist States. Edited by Archie Brown and Jack Gray. (London: Macmillan, 1976; New York: Holmes and Meier, 1977. Pp. xiii + 286. $24.00.)

This important book examines the interaction between political culture and political change in the USSR, Yugoslavia, Poland, Hungary, Czechoslovakia, China, and Cuba. Although the title recalls Lucian Pye and Sidney Verba's *Political Culture and Political Development* (1965), the latter should not be taken as a model for the present book. The authors acknowledge their debt to Pye and Verba, as well as to Gabriel Almond, but they quite strongly reject the notion of political development and instead emphasize the less value-laden dichotomy of continuity and change.

The definition of political culture employed in this volume is a controversial one, focusing on the cognitive and attitudinal elements familiar in the works of Almond, Verba, and Pye. This approach has been criticized by Geertz and others as one reflecting an overly subjectivistic and reductionist interpretation of culture. One might argue that the word "culture" is indeed misused when such factors as language, symbols, and rituals are omitted from its purview; political anthropologists have long recognized the importance of these cultural factors in political life. These comments aside, the contributors to this volume are consistent in their adherence to the definition and scope of the problem as presented.

The authors share the viewpoint that divergent patterns of political behavior in Communist countries are best explained by the differences in national political cultures. Despite a common ideology, similar institutions, and similarities in the ruling groups' approaches to political socialization, political behavior in Communist states continues to be influenced by patterns of beliefs and expectations inherited from widely divergent national pasts. The book argues, further, that Communists have most successfully evoked public acceptance of their rule where the prevailing norms of rule are most congruent with the traditional political culture (as in the Soviet Union), and least successfully where they seriously transgress against past political traditions (as in Czechoslovakia). A useful distinction is drawn between "official" and "dominant" political cultures to help determine congruence between the regimes' norms and citizens' orientations.

Like earlier books on comparative political culture, this one raises more questions than it answers. Unexplained, for example, are the psychosocial processes through which people's perceptions of their past condition their attitudes toward today's realities; this question evokes important theoretical problems of cognition and apperception, not to mention the complicated linkage between micro- and macropolitics within a society. (The authors explicitly reject systems analysis, but this does not entirely excuse them from addressing these problems.) Another issue not adequately discussed is the specific experience of Stalinism common to all countries studied (excepting Cuba) and its particular effect on the shaping of Communist political cultures.

Significantly, the authors are aware of the importance of political subcultures, and several of them attempt to account for their role. The special attention given to the multinational Communist societies is welcome; Stephen White's treatment of nationality in the USSR is especially well taken, as is David Dyker's brief but interesting survey of the various national traditions in Yugoslav politics. On the other hand, the reader is not offered a systematic theoretical discussion of the relationship between subcultures and the dominant culture, nor advised how to tell one from the other. For example, one must be skeptical about the implication that Czechs and Slovaks share a fairly coherent political culture notwithstanding political and social backgrounds that are different in certain fundamental ways. Nor do we find much discussion of subcultures arising from factors other than nationality. The question of social stratification comes up in every chapter, and the authors agree that the societies in question are generally quite sharply stratified (with the possible exceptions of China and Cuba). The reader might therefore expect to find significant differentiations in attitudinal patterns resulting from social structure, but only in White's chapter on the USSR are such patterns accorded more than a marginal influence on the political culture.

One should not dwell too long on the weaknesses of this ambitious volume, for its strengths are many. The book is well conceived and coherently organized. The individual contributions follow a standardized outline, yielding country studies that are genuinely comparable. The authors have utilized a variety of relevant data sources, including both opinion surveys (where available) and more indirect readings of political orientations. The amount and usefulness of survey data vary greatly from country to country, but in cases where they are few or lacking the authors compensate by the resourceful use of other evidence. The fine concluding chapter by Jack Gray, together with Archie Brown's introduction, offer a rich source of propositions for those interested in following up this line of research.

The question of political culture has provoked great interest and some controversy among social scientists in the past two decades, but few have been willing to tackle the theoretical and methodological problems it has raised. The authors of this volume, most of them quite young, have given us a provocative book that, let us hope, will stimulate a new round of serious discussion and research.

DAVID W. PAUL

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Light Water: How the Nuclear Dream Dissolved. By Irvin C. Bupp and Jean-Claude Derian. (New York: Basic Books, 1978. Pp. xii + 241. $10.00.)

The Three Wise Men, the Great Bandwagon Market and the Trojan Horse—these are some of the catch phrases by which Irvin Bupp and Jean-Claude Derian explain the undeserved triumph of the light water reactor over its heavy water and gas-graphite alternatives. Why did this particular technology come to dominate the world market? The authors conclude that light water's victory came from politics rather than economic efficiency. Their book examines the interaction between the European and American nuclear markets, paying special attention to France, Sweden and the United States. Their thesis is that the American Atoms for Peace foreign policy program led the Europeans to adopt the light water reactor prematurely. Light water was not a proven technology; it might not have been the right choice.

Clear exposition is the book's greatest strength. It diagrams the nuclear fuel cycle (p. 18), distinguishes between light and heavy water (p. 5) and explains how the Rasmussen Report determined the risk of a catastrophic explosion (pp. 122–23). Transatlantic political comparison is another strength. It finds the French are more willing than Americans to favor centralized, technocratic decision making. On the other hand French opponents of nuclear power are more apt to integrate their opposition into a comprehensive program of social radicalism. Swedes are more likely to debate nuclear policy in the legislature, whereas Americans use the courts or popular votes such as the 1976 California initiative (which the authors incorrectly call a referendum).

While these transatlantic comparisons are good, they could often be better. Many times a point is not made explicitly enough, which leaves the reader unsure, and denies the authors the opportunity to trace out the implications of their presumed conclusions. For example, do the authors believe that General de Gaulle in October, 1945 really intended to establish a secret nuclear weapons program, one that he activated when he returned to power in 1958? Perhaps so, but it the wily general had been that prescient, his astute foresight deserves more thorough discussion. Other small blemishes include abrupt transitions and questionable information about non-nuclear energy. On page 67 one suddenly learns that "most European manufacturers had shifted to light water reactors." When? Why? Again why, on page 137, is Sweden the logical country with which to compare the United States? And contrary to statements on pages 57 and 74 between 1960 and 1970 the United States was not self-sufficient in oil, in 1959 its oil imports came from Venezuela, not the Persian Gulf, and TVA's territorial boundaries came from legislation and hydro power, not coal.

Notwithstanding these minor flaws, *Light Water* makes a valuable contribution to the science, technology and industry literature that is developing all too slowly. Although political scientists have long taught about sub-governments and "iron triangles" that dominate Western liberal governments, they have slighted the private components—scientists, engineers and business executives. Bupp and Derian have dared to grapple with the complexities of the private and semi-private side. They have confronted the political role of General Electric, Westinghouse, Electricité de France and Centrala Driftsdningen. This is probably a more difficult intellectual challenge than nuclear physics.

DAVID HOWARD DAVIS

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Contemporary Japanese Budget Politics. By John Creighton Campbell. (Berkeley: University of California Press, 1977. Pp. xv + 308. $15.75.)

This is an excellent book. In this first major study of budgeting in Japan, John Campbell achieves a level of sophistication in analysis hitherto absent in policy research on Japanese politics. Budgeting is, after all, the major consistent policy-making activity of the Japanese government. Campbell captures the complex interplay between multiple political actors in this important process, compares his results with those from American and European research, and contrasts differences in budget-making styles across a 20-year span between 1954 and 1974. Campbell's book also suggests new ways of conceptualizing Japanese policy making that will undoubtedly assist future researchers of the still little-studied processes of national politics in that country.

Somewhat akin to Lowi's speculative identification of policy arenas, Campbell's empirical analysis identifies four major phases of budget making wherein different combinations of political actors are active. The phases are micro-budgeting, macro-budgeting, integration and revival negotiations. Through this conceptual device Campbell is able to untangle what would otherwise be unmanageable complexity in actor participation, actor strategies and budget decision-making criteria.

Japanese budget policies involve multiple political actors: the Ministry of Finance, the other government ministries and agencies, the Liberal Democratic party leadership, rank-and-file Liberal Democratic Diet members, and, of course, interest groups and local interests. Rank-and-file Diet members, Liberal Democratic party middle-range policy-making groups, middle-level bureaucrats, and, in varying forms local and national interests are participants in *micro-budgeting* activities. In this phase of the budget-making scenario individual ministries present their requests for funding to the Ministry of Finance. In contrast, *macro-budgeting* involves leading Ministry of Finance officials, the Economic Planning Agency and other actors whose principal concerns are the overall size and economic effects of the budget. In the *integration* phase of budgeting, micro- and macro-budgeting parameters are mutually adjusted (i.e., aggregation), and in the *revival* negotiations ministerial and Liberal Democratic participants make last-ditch efforts on behalf of their priority expenditure demands.

Campbell's analysis is rich in its delineation of the congeries of actors present in each phase of budget making, and adds to recent studies by Haruhiro Fukui and others which once and for all reject the simplistic notion that Japanese politics is run by a triad of monolithic actors: the bureaucracy, the Liberal Democrats and big business.

Campbell's book is also rich in its insight into the strategies of various political actors and the criteria by which Ministry of Finance bureaucrats make budgeting decisions. In micro-budgeting, the criteria are incrementalism, simplification, resistance to new projects, relegation of difficult decisions to later phases of the budget process and "balance." The concept of balance, i.e., giving different ministries more or less equal proportions of increased funding, is in Campbell's view a Japanese cultural component, whereas many other aspects of budgeting processes are similar to their counterparts in other political systems. In macro-budgeting the criteria are fiscal policy, economic growth and revenue estimates as well as other broad economic issues.

What distinguishes Campbell's analysis of actor strategies and budget-making criteria is his unusual sensitivity to the manipulative politics everpresent in interactions between different political actors. Particularly fascinating is Campbell's discussion of the Ministry of Finance's manipulation of information and attention to timing in its efforts to maintain control of the budget-making activities.

Campbell also carefully analyzes the role of the long-dominant Liberal Democratic party in budget making. He suggests that the party is most effective generally in the micro-budgeting process, where rank-and-file Diet members and various middle-range policy organs within the party interact intimately with counterparts in government ministries. In micro-budgeting the party actors function mainly as articulators of local and functional interests.

The party's higher organs and leadership groups also make contributions to the budgeting process. But their efforts are not consistently effective. Some of their budget statements are too general to be meaningful guides for action, while lists of specific budget request items submitted by the party's policy groups and the party leadership are too unaggregated to serve as useful indicators of funding priorities. The reason for this state of affairs lies both in the internal politics of the party, and in the Liberal Democrats' lack of ability to cope with the complexity of many budget issues. However, some Liberal Democratic leaders and Prime Ministers have intervened in budget processes successfully, either by virtue of their own prior bureaucratic experience or as a result of their personal political style.

Actually, it is difficult to portray in a review the sophistication and depth which characterizes Campbell's book. His study of budgeting is literally rich with insight into the complexity of real political life, yet so clear in its delineation of actor motivation and bureaucratic behavior that it provides a major breakthrough in our understanding of Japanese politics. It attempts no oversimplification for the purpose of building policy process models, which some political scientists might prefer. But this is actually the strength of Campbell's book, i.e., its portrayal of the real and complex world of Japanese politics at a time when we do not yet know enough to begin the development of more general policy models. *Contemporary Japanese Budget Politics* is a major contribution to American political science research on Japanese politics.

BRADLEY M. RICHARDSON

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Manipulating Soviet Population Resources. By Jeff Chinn. (New York: Holmes and Meier, 1977. Pp. vii + 163. $22.50.)

This book, the fruit of two years' labor in the Soviet archives, confronts two main problems of urban population policy in the Soviet Union: raising the birth rate and redistributing the population by directing migration to labor-short areas (mainly in the west) of the USSR. These issues are examined in the context of some political science theory (largely centering on Deutsch's cluster variables) and the enormous amounts of data that only the archivist can amass relative to several Soviet cities: Moscow, Leningrad, Kiev, Kharkov and Riga.

Chinn appears to attempt too many things in too few pages. If his dissertation had to be significantly cut in order to be published at a relatively affordable price, the publishers should have helped him with better editorial assistance. For as it is, the data presentation, descriptive material and, to some extent, the demographic and economic analysis consume so much space that only tantalizing pieces of political science analysis are presented. Let me hasten to add, however, that these analyses are so thought-provoking that one wishes for Jeff Chinn an additional forum for their elaboration.

For example, in a first-rate discussion of the problem of bureaucratic competition within the Soviet system, Chinn notes the existence of a three-part process in any country whereby "(1) policy influences (2) certain socio-economic factors which in turn influence (3) birthrate" (p. 54). Fortunately or unfortunately, "policy relating to socio-economic factors is not made solely to influence the birthrate" (p. 54). Thus, demographers might wish to keep down the percentage of women employed full-time or attaining college degrees because these factors correlate negatively with birthrate. However, economists or factory managers might aim at increasing the educational and career orientation of women in order to relieve the burden on the already labor-short Soviet system. These competing goals will probably never be formally reconciled. Chinn notes on the first page of his book that "because the Soviet political system undertakes long-range economic planning, comprehensive population policy is required." No modern industrial society has been able to reconcile the many competing claims, interests and ideological perspectives involved in establishing an integrated population program. The Soviet Union is no exception.

In fact, ironically the USSR may have a more difficult time than Western societies in formulating a demographic program. First of all, the USSR will confront all of the problems of coordination, control and scale that are typical of any huge country. Second, there is the layer of problems long considered by western sovietologists to be moot. We now know that there are problems such as chauvinism or slippage between the enunciation of a central directive in Moscow and its enactment in the republics and provinces to which the Soviet system has hardly been immune.

Finally, there is an entire set of difficulties attendant to population policy formation that are unique to the USSR, or at most, a few other communist states. Thus, there are significant problems with data availability and comparability because of a long-standing suspicion of social science methodology in the USSR and because data gleaned from the censuses are not always entirely shared with scholars and statisticians. Then there are the difficulties produced by the ideologization of so many social and economic issues. As Chinn notes, the influence of ideology on urban policy has been very pronounced in the USSR. Views adopted from Marx and Engels which regard cities as "superior" to rural areas have strongly affected Soviet attitudes toward urban growth. These attitudes are very difficult to counter, even by those who can prove that birthrates decline as urbanization increases.

The last set of unique difficulties relates to the low prestige of demographers in the Soviet Union. This low prestige is caused, in part, by an unfortunate history whereby certain census compilers lost their lives in the 1930s because Stalin was displeased with the glaring evidence of death caused by the purges. Another factor is the high status of economists. The end result is that "demographers, arguing that immediate action must be taken to remedy a long-term problem, are at a disadvantage when contending with economists who are more concerned with current issues" (p. 53).

Because the Soviet Union, like the United States, is oriented toward the present, it is not prepared to make the massive investment in the future that paying women to stay home and have children would entail. Furthermore, because the USSR, again like the U.S., is a male-oriented society, it is not prepared to make the demographic investments and the transition to greater part-time employment that working mothers need. It is true, as Chinn notes on p. 126, that modernization "alters women's roles even more fundamentally than those of men." Soviet-style modernization, with its low emphasis on the service sector, has presented Soviet women with an even more extreme choice between children and career than is true in most post-industrial societies.

HELEN DESFOSSES

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P'yongyang Between Peking and Moscow: North Korea's Involvement in the Sino-Soviet Dispute, 1958–1975. By Chin O. Chung. (University, Ala.: University of Alabama Press, 1978. Pp. x + 230. $15.00.)

The dispute between the People's Republic of China (PRC) and the Soviet Union, now two decades old, is one of the most momentous developments in the history of international Communism. Not only has it played a key role in the fragmentation of the Communist world, but also it has been and continues to be a major factor in contemporary international politics. The Sino-American rapprochement of the early 70s and the normalization of Sino-Japanese relations which culminated in the conclusion of a peace treaty between the two erstwhile enemies in 1978, for example, might not have taken place had it not been for the Sino-Soviet dispute.

Among the countries which have been particularly affected by the unabated rivalry and conflict between the two Communist powers are those on their periphery. The Democratic People's Republic of Korea (DPRK) is a case in point. In addition to sharing borders with them, the DPRK has close historical, political, economic, and military ties with both the PRC and the Soviet Union. Add to the above the perceived politico-strategic needs of the two Communist giants, neither of which has shown any willingness to allow the other to assert hegemony over P'yongyang. Torn between the horns of a dilemma, North Korea has opted for the strategy of cautiously walking on two legs, so to speak. That is to say, it has made a virtue of necessity by pursuing the policy of flexibility—maintaining neutrality if possible, taking sides if necessary, and unabashedly pursuing maximum payoffs in economic, technical, and military aid at all times.

The book under review is a detailed case study of how the DPRK has performed what can fairly be described as a considerable feat. For what P'yongyang has achieved is not only to circumvent the adverse effects of the schism between its two powerful neighbors and patron states but also to utilize the dynamics of the Sino-Soviet rivalry to bolster both its internal capabilities and international standing. In other words, instead of being crushed—an old Korean saying goes, "When the whales fight, the shrimp gets crushed"—the DPRK has managed to assert a growing measure of independence in the world arena while simultaneously receiving a sizable amount of aid from both of the contending powers.

Based on "official statements and speeches of the leaders of North Korea, the PRC, and the Soviet Union; newspapers, articles, and books published by the three Communist regimes; and secondary sources published in the United States, South Korea, and Japan" (p. 3), this is a chronological account of North Korea's response to the shifting currents of the Sino-Soviet dispute in the 1958–1975 period. In addition to a brief introduction and conclusion, the book is divided into eight substantive chapters. The first chapter provides historical background, while the remaining chapters deal with the various stages in the triangular relationship—e.g., the period of neutrality, the period of solidarity with Peking, the period of rapprochement with Moscow, etc. The author combines a descriptive narrative with an interpretative analysis; whenever a shift in North Korea's posture occurs, the author does not shrink from offering speculative hypotheses regarding the possible reasons for the change.

By and large, the story which Chin O. Chung tells is familiar to the student of North Korea and is readily available in the English-language literature on North Korea, as the book's extensive bibliography and footnotes amply demonstrate. To say this, however, is not to detract from the value of the book. For not only is its coverage of the post-1970 period quite thorough, but the presentation of an in-depth discussion of the triangular relationship in one convenient volume may also be a distinct service to experts.

On the other hand, the book suffers from the usual limitations of a case study. What are some of the larger implications of the author's findings? What may non-Korea specialists or comparativists learn from the study? Is there any linkage between the author's findings and any theoretical propositions concerning foreign policy behavior? Anyone interested in such questions will be disappointed. In fairness to the author, I must hasten to point out that the above is less a criticism of this particular study than a commentary on the limitations of case studies in general.

In sum, insofar as the author has the limited goal of describing and interpreting P'yongyang's behavior in the unfolding drama of the Sino-Soviet conflict, he has amply fulfilled his goal.

B. C. KOH

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Society against the State: The Leader as Servant and the Humane Uses of Power Among the Indians of the Americas. By Pierre Clastres. (New York: Urizen Press, 1977. Pp. 186. $12.95.)

The work of Pierre Clastres is a good introduction to the most recent critique of contemporary social science for those who lack the background or the stamina to tackle the neo-Hegelian writings of Habermas or the "new philosophy" of French writers like Lefort. This is not to say that Clastres' work is non-theoretical or drily empirical. It is a fine, reflective essay drawing from several philosophical traditions and academic disciplines. *Society Against the State,* an anthropological study of several Amerindian tribes, attempts to reveal the ideological weaknesses of the dominant camps in Western ethnology as well as their left-wing critics. Both commit the sin of reductionism: the one by reducing politics to coercion and the other by reducing it to the economic.

For classical ethnology, Indian societies are archaic or primitive, principally because the economies of these societies are on a subsistence level. For Clastres this is not a factual appraisal, but a value judgment that betrays an ideology masked as value neutrality. To say that an economy functions at the level of subsistence is to say that the society hangs in a permanently fragile equilibrium between basic alimentary needs and the means to satisfy them. Archaic societies, then, simply survive, constantly threatened by the slightest natural accident. They are culturally and technologically incapable of producing a surplus, entirely absorbed as they are in producing the minimum necessary for subsistence. But if one looks at technics as Clastres does—as the set of procedures human beings acquire not to insure an absolute mastery of nature (the "insane Cartesian project," p. 161), but one suited and relative to their needs, there is no longer any reason to impute technical inferiority to primitive societies. Though little time is spent in work (Guarani males till the soil about two months every four years!), there is no anxious, full-time search for food. Instead, the men spend their time in pleasurable pursuits: hunting, fishing, drinking, fighting.

It would be senseless for these societies to produce a surplus, not only because the needs of the individual have already been met, but because there is no state apparatus to distribute surplus wealth to unproductive groups. To the ethnologist, primitive societies *are* societies without a state (witness their greater appreciation of the "civilization" of Montezuma's Mexico or Peru of the Incas). The absence of the state is taken as the absence of civilization; primitive societies are said to stagnate in a pre-political or anarchic historical stage. This brings us to the heart of Clastres' critique of ethnology as ideology: it illicitly identifies political power with coercion and subordination and takes as its model of the political the institutions characteristic of Western civilization. Clastres does not assert that power is absent in primitive cultures, but that it exists separate from violence and apart from hierarchy. All societies, archaic or not, are political, but not all are coercive. In many, holders of what elsewhere would be called power are actually without power, since no relation of command-obedience is in force. The idea of giving an order or of having to obey one is completely alien to an Indian, except under very special circumstances. The chief is a stranger to the essence of the institution he appears to represent—authority. He is never certain that his "orders" will be carried out, for his power depends on the good will of the group. "The chief's word carries no force of law" (p. 175). As a consequence, his principal concern is maintaining peace, for the breakdown of internal harmony weakens the prestige normally his.

Clastres gives flesh to the thesis that non-coercive political power exists by examining sexual roles in Amerindian societies, as well as the function of humor, language, religious prophecy, torture and writing (or at least the marking of the body). The analyses are all fascinating—no other word will do, since so often Clastres' descriptions transfix through terror. The scars left on the body during initiation rites, for example, become the inscribed text of primitive law. They are a terrible cruelty designed to prevent the advent of a more terrifying cruelty, a law separated from society, hovering over the individual and used by one group to dominate the rest. The law the Indian comes to know through pain and which he carries on his body says to everyone: "You are worth no more than anyone else; you are worth no less than anyone else" (p. 156).

Clastres goes on to criticize Marx for his inability to explain the transition from non-history to historicity, from noncoercive power to the legitimated violence of Western state apparatus. At least in primitive societies a transformation of the economic infrastructure does not automatically bring about a change in the political superstructure. Clastres points to several different societies with the same infrastructures but different superstructures, and several others with the same superstructure but fundamentally different economies. It is not material change which is decisive in transforming the primitive into the proto-modern, but political revolution, "that mysterious emergence–irreversible, fatal to primitive societies–of the thing we know by the name of the State" (p. 171).

By claiming that the political follows a dynamic independent of the material, Clastres concretizes the assertions of Habermas and the new philosophers that language (intersubjective meaning) and social liberation cannot be explained by a theory whose principal category is labor. Clastres' critique of Marx and his assertion that primitive societies are noncoercive are considerably weakened by the fact that the societies he describes already depend on a rigid division of labor: that men need work only two months in four years is made possible by the continuous labor of women, something which Clastres inexplicably ignores. Nonetheless, *Society Against the State* contains a wealth of information and critical insight, the value of which cannot be gainsaid.

GEORGE ELLARD

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Public Opinion in European Socialist Systems. By Walter D. Connor and Zvi Y. Gitelman, with Adaline Huszczo and Robert Blumstock. (New York: Praeger, 1977. Pp. ix + 196. $17.50.)

The contributors to *Public Opinion in European Socialist Systems* examine the emergence of opinion polling in four states (Poland, Adaline Huszczo; Czechoslovakia, Zvi Gitelman; the Soviet Union, Walter D. Connor; Hungary, Robert Blumstock), the changing political context, and the content of certain polls from the sixties (late fifties for Poland) to early in the seventies. An introductory chapter by Gitelman addresses the function and history of opinion research in the four countries and a concluding chapter by Connor pulls together the main findings. Since surveys there are constrained from investigating explicitly political issues, the findings are most illuminating when related to politically relevant sociodemographic processes. Blumstock's section on Hungary is a particularly good example, and his analysis of the crucial interrelationship between attitudes on social inequality and the implementation of the New Economic Mechanism provides genuine insights into the ambivalent Hungarian reforms. The authors are all very good in their analyses of national political shifts and the resulting impact on the kinds and scope of permitted sociological research. However, their very sensitivity to these issues tends to obscure the significance of the surveys themselves, both as sources of information on attitudes in socialist systems and as activities conducted by a professional elite. The authors overstress the manipulative function of opinion research in these countries, comparing it to market research in our own system. Opinion formation as a function of surveys in *nonsocialist* systems is not limited to the economic sector, and the rhetoric of polling in *socialist* systems is not the reality. Since, in socialist systems, the rationalizing rhetoric of surveys must relate to the functional utility of the research in the fulfillment of policy directives, the Western observer should push beyond the mandatory ritualistic shell in which sociologists report their findings.

The findings of the surveys are interesting, although the reader would profit from a more consistent and rigorous description of the methods, including sample size, when available. Further, it is often difficult to follow comparisons of surveys, as, for example, when Huszczo compares attitudes of emigre Poles interviewed in the West in 1952 and Poles surveyed in Poland by Poles in 1963; it is not sufficient to say: "Whatever the validity or comparability of these findings, it is clear that Polish attitudes were of a much different character by the 1960s" (p. 65). Obviously the question of methodology, both of this author's use of surveys and of the surveys themselves, presents problems. In a very general way, the authors fault socialist sociologists for methodological deficiencies in the presentation of survey results, sample construction, and interpretation of the "no-answer" response. Readers who wish to examine these problems further should consult two books not cited in the present study: Elizabeth Weinberg, *The Development of Sociology in the Soviet Union* (London: Routledge and Kegan Paul, 174) and Mervyn Matthews, *Class and Society in Soviet Russia* (New York: Walker, 1972) as well as the monographs by David Lane.

The question of methodology is central to an appraisal of the prospects for socialist survey research. Connor, on the Soviet Union, is pessimistic about the future of opinion research, calling it virtually "invisible" at present, and about the possibility of significant improvement in methods. Certainly, early polling attempts were very crude (and occupy far too much space in the present book), but he overlooks the impressive leaps in theoretical sophistication we have witnessed in the seventies. The eminent Soviet sociologist, V. E. Shliapentokh, has set standards for the field there with his two comprehensive overviews, informed by a very wide knowledge of current research, both East and West: *Problems of Reliability of Statistical Information in Sociological Research* (Moscow, Statistika, 1973) and *Problems of Representativeness of Sociological Information* (Moscow, Statistika, 1976). Increasingly, recent polling in the Soviet Union displays a more rigorous and self-conscious underpinning, particularly in the areas of mass media research, urban migration, labor transience, and the family. These and other types of surveys are reported in books and also in a source that is, curiously, never mentioned in the book under review: *Sociological Research*, the prestigious journal of the Academy of Sciences' Institute of Sociological Research.

This book is evidence of a growing Western interest in socialist surveys, through which we may see, especially in Blumstock's treatment of Hungary, glimpses of profound social relationships.

ELLEN MICKIEWICZ

Michigan State University

Liberal Party Politics in Britain. By Arthur Cyr. (New Brunswick, N.J.: Transaction, Inc., 1977. Pp. 318. $12.95.)

Approximately three decades ago, a member of the Labor party accused the Liberal party of being like the bull in *Tristram Shandy*, of going about its duties with so brave a face that it was credited with powers which had long ago vanished. The contemporary Liberal party suffers from the absence of an organized group loyalty and its inability to secure the services of a powerful economic patron, languishing—as Phil Fothergill used to say—in honest or ineffective poverty. Seldom can the party claim more than scarcely a dozen parliamentary seats. The party is, nonetheless, the symbol of a distinguished heritage whose past has at times been glorious, imparting to the parliamentary process a tone and dignity that have been identified with the best values in British political life.

Liberal Party Politics in Britain inquires into Liberal party history and doctrine, the leadership styles of varous Liberal leaders (especially Jo Grimond and Jeremy Thorpe, and to a lesser extent Clement Davies), and the most important transformations that have occurred within the party in recent years. The research undertaken consists in part of interviews with Liberal party members, both leading and middle-level, and a written questionnaire circulated among 157 Liberal party activists (to which 104 responded). The initial version of the study was prepared as a doctoral dissertation under the supervision of Samuel Beer. Michael Steed, Liberal party member and lecturer in the Department of Government in Manchester University, has authored a foreword.

Cyr views as altogether improbable restoration of the Liberal party as a major political force. Numerous social and political changes presently affecting the scene do not promise to redound to the advantage of the Liberal party in an era characterized by movement away from national parties. Moreover, prospects for future Liberal party advancement are dimmed by the rise, primarily in Scotland and to a much lesser extent in Wales, of regional parties. The Liberal party, for example, is hardly a match for the Scottish Nationalist party, which reflects in the selection of its parliamentary candidates less of an anti-working-class bias, and which promises its electorate that it will go all the way towards attainment of Scottish independence, pushing into the background Liberal representations in behalf of administrative decentralization and greater political freedom for Scotland. Finally, the Liberal party simply is too middle-class, and various of its overtures to other social categories fall flat, being out of harmony with British class relationships. Workers and trade-unionists shun the organization and Liberal pleas to them for participation in profit-sharing, for example, do not go well with British labor's conception of management.

Although the quality of Liberal party leadership has been other than uniformly good, Cyr writes appreciatively of Jo Grimond, who introduced to the organization new ideas, new people, and a recognition of the necessity of reform. Grimond helped develop Liberal credibility and during his time the term "Liberal revival" grew substantially. He knew how to deal with mass media—particularly television—and with party unity (although admittedly few of his efforts were devoted to resolving internal conflicts). His was a style of leadership reminiscent in some ways of the nineteenth century, and an excellent rebuttal to those inclined to under-assess the influence of personality in politics. Clement Davies, Grimond's predecessor, receives slight praise, and Jeremy Thorpe, Grimond's successor, considerable criticism, although Cyr acknowledges that both men were confronted by different situations during their leadership periods. Thorpe's tenure was a time of internal dissension, renewed electoral regression, financial crisis, deficiencies in central party organization, and, periodically, warfare between the party executive and the parliamentary party.

The Liberal party lacks wealth and extensive public support, and frequently its administrative structure is unstable; yet, the organization has been able to survive, and primarily because of the existence of a Liberal establishment that acts as sort of a "continuing anchor." However, this establishment has been unable to engineer a real revival based on a stable and loyal voter clientele, one that comprises people other than those disgruntled temporarily with the major parties and who have only a fragile commitment to the Liberal party.

Cyr has authored a work that is both readable and enjoyable. Some readers may find some of his explanations occasionally (although infrequently) too neat and too pat—i.e., particularly his discussion of how Grimond's disdain for party administration did fit so well within Liberal party tradition—and others may raise now and then an eyebrow relative to the pace at which he regards class as diminishing (although not viewing it, certainly, as ready to bubble away in the sea). Nevertheless, these considerations are secondary in a commendable effort which, mercifully, examines the decline of the Liberal party without offering for that phenomenon yet another "definitive explanation."

LOWELL G. NOONAN

California State University, Northridge

Québec: The Unfinished Revolution. By Léon Dion. (Montréal and London: McGill-Queen's University Press, 1976. Pp. xiv + 218. $15.00, cloth; $7.50, paper.)

Choix: Le Nationalisme Québécois à la Croisée des Chemins. Centre Québécois de Relations Internationales, Institut Canadien des Affaires Internationales. (Québec: Université Laval, 1975. Pp. 382. Can. $5.00, paper.)

Victory of the separatist Parti Québécois in November 1976 opened a new era in Quebec's political history. Both of these books, while written before that event, provide valuable background for understanding Quebec society, the circumstances leading to the PQ victory, and its possible consequences.

Quebec: The Unfinished Revolution is a collection of essays, originally published in French as *La prochaine révolution.* The essays themselves appeared in a variety of popular and scholarly publications from as early as 1961. It is a reflection of Dion's quality as a political scientist that none is outdated, and a measure of his perceptivity as both scholar and engaged intellectual that we can learn from all of them.

The longest essay, published in French in 1975, but directed to the anglophone reader, describes four kinds of nationalism. Conservative nationalism was dominant from the nineteenth century until 1960; liberal nationalism took over with the liberal party victory and the beginning of the "quiet revolution" in 1960, while two variants of more radical forms of nationalism, described as social democratic and socialist, have come to the fore since 1965. The Parti Québécois exemplifies the social democratic model. Each form of nationalism is evaluated in terms of preconditions, arguments used, political impact, and political functions.

We come to understand, as we read this essay and others, that Dion is not primarily concerned with the nature of the political state and the forms it might take, but with the survival of a people. Each kind of nationalism then has merit insofar as it encouraged that survival, and each has its limits. Liberal nationalism, for example, contributed to an opening of Quebec society, but it also produced hopes and aspirations it could not fulfill, setting the stage for more radical approaches.

As a concerned intellectual, Dion affirms the uses of history to define the present of a nation or people. As a scholar, he searches history for those shaping influences and ideas that continue to have contemporary relevance. But he is selective. One could argue, for example, that French ties with their broader North American experience have been played down. This leads to an underemphasis on the French as founders of Canada, shaping it and being shaped in turn. At the same time, Dion underestimates the extent to which Canada can be primarily English-speaking without being primarily British. For those francophones who recognize this fact, it often becomes linked to their contempt for the British, who have not retained their hold as a people. At the same time, they see non-British origins as indicating lack of sympathy to francophone aspirations (with some truth), and desire to negotiate with the British as the only equal partners in the Canadian state. By not communicating the full nature of Canadian society, there is some gap in expressing what it means to be French Canadian, and not only Québécois. To some extent, francophones, like anglophones in North America, share characteristics of individualism, pragmatism, and personal ambition. Where these mass characteristics are sensed, Dion has a tendency to discount their significance, for example, by conceiving of nationalism as primarily an elite response. And yet, Dion can still conclude his final essay with an appeal for guidance from the values of the poor.

Le nationalisme québécois is also a collection of essays, but by 20 authors. It originated at a 1974 conference jointly sponsored by the World Peace Foundation of Boston, the Centre d'Etudes canadiennes-françaises of McGill University, and the Centre Québécois de relations internationales of Laval University. All the essays are concerned with Quebec nationalism, but from the perspective of relations among three capitals: Quebec, Ottawa, and Washington. What may appear is a rather peculiar set of emphases can be traced to the sponsorship, with its focus on international relations, particularly as these assume a transnational character. The approach is novel, though the overall intentions of the volume are not fully realized.

In an introductory essay, Legault briefly reviews the dominant issues and perspectives, not all of which are included in this volume. He considers three units of analysis—Quebec itself, Quebec in relation to the federal government, and Quebec in its foreign relations, almost solely with the United States. Within each unit questions are raised from a number of perspectives on nationalism: Marxism, political nationalism, economic nationalism, linguistic autonomy, and cultural identity. Marxism, represented in the essay by Latouche, is currently important because of the new direction of nationalism in Quebec, expressed to some degree by the Parti Québécois (but as Dion suggested, it is moving away from strongly radical commitments), but also by other groups further to the left. The Latouche and Pinard articles bring together the controversy over the measure and extent of separatist sentiments. Latouche feels popular support is widespread, analogous to support for the Parti Québécois. Pinard argues that the broadness of Parti Québécois support is based on considerations other than desire for separatism.

The emphasis on relations with the United States reflects a common Canadian concern. Since it is not customary to look at this in terms of a single province, essays on the subject are provocative, although at times they tell us little more than that Americans, both members of the elite and of the public, know next to nothing about Quebec, and that the U.S. government is not prepared to see Quebec as more than an internal Canadian problem.

For the reader who wants to know more about the content of federal-Quebec relations, the book will be disappointing. It is most satisfactory in talking about Quebec itself, and at least suggestive in considering future foreign relations. In this regard, the Bouvier and Donneur article can be recommended for its thorough consideration of possible scenarios.

Both books are successful in communicating the ferment and excitement of Quebec society today. Written before the advent of the Parti Québécois government, these books still manage to take seriously the issues that led to that election, and the possibilities and consequences of separation. Their conclusions are similar and should be regarded as an admonition. Rational considerations and economic well-being may argue for the preservation of Canada with its current boundaries; however, what makes a nation and holds it together is something more elusive and more emotional.

MILDRED A. SCHWARTZ

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China's Economic Revolution. By Alexander Eckstein. (New York: Cambridge University Press, 1977. Pp. xii + 340. $19.95, cloth; $6.95, paper.)

Completed just before Eckstein's untimely death in 1976, this useful book will serve either as a text for a college-level course on the Chinese economy or as a means whereby more advanced students and scholars from other disciplines can gain a familiarity with the details of the economic system of the People's Republic.

The book begins with a review of China's "economic heritage," wherein Eckstein sets out the meanings of the term, "under-development" and shows the ways in which the pre-Liberation Chinese economy fits, or fails to fit, into that category. Of particular interest here, as well as in chapter 5, is his discussion of the nature and effects of the rampant inflation characteristic of the economy during the republican period (1927–49). The second chapter—devoted to "development strategies and policies"—is, in my estimation, least successful. Eckstein appears to have been least comfortable when dealing with questions of political decision making and ideological disputation, and the product does little to shed new light on these problems. Chapters 3 and 4, by contrast, offer a particularly clear and informative review of the structure and operation of the economy.

The following three chapters are devoted to an exploration of several of the goals toward which the author sees Chinese economic development as proceeding—stability, growth, self-reliance, an improved standard of living and a more equitable distribution of economic benefits. The final chapter summarizes the argument by describing the Chinese "developmental model," evaluating the degree to which it has been effective in realizing its own goals and discussing the applicability of this model to the economic development of other underdeveloped areas.

It is this last chapter which reveals with the greatest clarity the degree to which Eckstein's view of the Chinese economic system evolved during the course of his distinguished career as a scholar and teacher. Whereas his earlier work is characterized by a rather strong skepticism concerning the logic of Chinese development, and by a tendency to evaluate Chinese performance exclusively on the basis of non-Chinese criteria, this book, by contrast, manifests a serious attempt to take the Chinese on their own terms and to include those terms among the criteria on the basis of which their economic performance is evaluated. Particularly striking in this connection are his comments on the Great Leap Forward (p. 286) which he came to see as a highly rational response to China's peculiar resource mix. It was, he says, an attempt to augment scarce capital and cultivable land through maximization of the abundant factor—labor power. Although misapplied at first, through trial and error, the strategy has been perfected to the point of contributing substantially to what he sees as, on balance, an impressive developmental performance since Liberation. He comes close to suggesting the usefulness of a dialectical approach to his subject, but falls short of it by neglecting to show how successive policies evolved out of both the positive and negative lessons derived from the strengths and weaknesses of earlier policies. This shortcoming is perhaps best examplified by his treatment of the move from the strategies of the First Five Year Plan to those of the Great Leap (pp. 50–58, 197–202).

Derived from lecture notes, the book appears to have been intended primarily for classroom use. Its weaknesses as a text derive first from the occasionally idiosyncratic sequence in which topics are treated and secondly from the insufficient annotation and bibliographic guidance provided the student reader. Both weaknesses are, no doubt, attributable to the fact that Eckstein died while the book was in production, and both are easily compensated for by means of a carefully constructed syllabus.

As Eckstein himself suggested (p. 317), 1975–76 was, in some respects, an unfortunate moment to have completed this book. Several important shifts of policy—policies affecting education, technical and scientific research, farm mechanization and the importation of foreign equipment and technical assistance—have occurred in the wake of Mao's death and the arrest and criticism of the "Gang of Four." While the author prepared his readers for the possible effects of such changes of policy, his comments on the changes themselves would have been even more illuminating. We are fortunate, nonetheless, to have the book as it stands. It serves as an appropriate reminder of an instructor and scholar who continued to learn as he taught.

JOHN BRYAN STARR

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The Brazilian Corporative State and Working-Class Politics. By Kenneth Paul Erickson. (Berkeley: University of California Press. Pp. xvii + 225. $14.00.)

Since at least 1930 organized labor has been a critical actor in the Brazilian political process. All governments during this period have expended considerable energy seeking to control labor either through policies aimed at eliciting positive support or, as in the present regime, through systematic and often violent coercion. During the authoritarian reign of Getulio Vargas (1930–1945), labor relations were organized within a corporatist structure designed to endow the administrative apparatus of the federal state with an autonomous capacity to control labor. This structure has remained largely intact down to the present. The topic of working-class politics in Brazil therefore cuts to the core not only of modern Brazilian politics, but also of a number of theoretical and conceptual issues of interest to a substantial number of political scientists: namely, authoritarianism, corporatism and the role of the state in modern developed and developing capitalist societies. This book is therefore quite timely. Happily, it is also well conceived and executed.

Erickson allies himself with those who have recently argued for the importance of recognizing corporatism as a third "ism" (beyond Marxism and Liberalism) from which can be drawn an important analytical perspective on modern Western politics in general and Latin American politics in particular. Corporatism, viewed as an ideology and a concrete set of political structures linking the state and the working class, is therefore the central explanatory theme within which Erikson explores various historical and contemporary dimensions of Brazilian working-class politics. While there are no theoretical breakthroughs in the book, our conceptual feel for this theoretical approach is heightened by following Erickson's skillful use of it to structure and inform his case study, which in turn is a solid contribution to our understanding of working-class politics in Brazil.

Following from his approach, Erickson is appropriately sensitive to the role of legal and administrative structures in channeling and organizing the political process. Thus, aside from the usual type of material on politics as group dynamics of "process," the study contains important and insightful material on the legal foundations of labor politics in Brazil and the critical role of the Ministry of Labor in Brazilian politics. We also get a good sense of how in Brazil, prior to 1964 at least, labor leaders deployed resources such as patronage and political strikes to enhance their power in the overall political system.

The central conclusion of this book, however, is that corporate structures effectively forestall the capacity of labor to act as a creative and progressive force in the political process. They do so primarily by divorcing the labor leadership from its base and converting it into a bureaucratic appendage of the state. Thus while in certain circumstances, such as those that prevailed in Brazil from 1945 to 1964, labor leaders can create the illusion of being a political force in their own right, in the end their power rests in the state apparatus and not in an autonomous and coherent set of labor organizations. Hence, when the state came under the control of groups hostile to labor, as happened in 1964, it was a relatively easy task to undercut and/or eliminate the existing stratum of labor leaders and submit the mass of organized workers to the firm control of the central authorities who then imposed on labor a substantial portion of the costs of the subsequent process of economic development. In analyzing how this state of affairs came about, Erickson shows a steady and solid grasp of the complexity of labor politics in Brazil and in so doing has produced a most satisfactory book.

JAMES M. MALLOY

University of Pittsburgh

Political Parties and Elections in the French Fifth Republic. By J. R. Frears. (New York: St. Martin's Press, 1977. Pp. xii + 292. $17.95.)

The problem of unity threatens the stability of French government in three major ways. First, the four major parties which now exist in France must secure the unity of their often-heterogeneous elite members. Second, both the Left and the Right consist of a coalition of two major parties. Unity depends on the continued cooperation of these partners. Finally, unity rests on the ability of these coalitions to attract the support of a reasonably loyal electorate. In his book, J. R. Frears gives us a traditional, useful and up-to-date discussion of the history of and prospects for each of these problems of unity.

On the right, the unity of the gaullist party (currently the RPR) seems assured since the emergence of Jacques Chirac as its leader in late 1976. Not so with the coalition which centers in the Republican party (PR). The focus of this coalition is its support for President Giscard d'Estaing, and it remains a highly heterogeneous grouping of splinters. Giscard's own PR holds about half the coalition's seats; the others belong mainly to former radicals (Place Valois) and Christian Democrats (Lecanuet). Clearly, this grouping is not yet a party. Many of its members have joined for opportunistic reasons, the political ground in the center having simply vanished. Whether Giscard could hold this group together, should a centrist alternative once again emerge, remains uncertain.

The major differences between the RPR and the giscardians are more in emphasis than in substance, and at this point, the unity of the Right is not seriously threatened. Giscard propounds a more centrist and European policy than did DeGaulle, and the PR generally views "the authoritarianism, the mysticism and the exaltation of the Nation associated with De Gaulle" (p. 64) with suspicion. Nonetheless, the majority has survived first the passing of De Gaulle, and now the disappearance of the gaullist presidency, signals which suggest that differences between the two camps remain within controllable boundaries.

The situation on the Left is similar, but more extreme. Unity has never been a problem with the Communists, of course. The Socialist party, however, has only recently emerged from a long period of disunity and decline. Francois Mitterrand has reversed this decline by institutionalizing diversity within the reorganized structure of the party. Because of this, there remain identifiable tendencies within the party which could split either toward the left or the center, should the opportunity arise.

By far the most intractable problem of unity is presented by the coalition of the parties of the Left. Beginning in the 1960s, the Socialists and Communists concluded a Common Program, an agreement which set out a number of policy positions, and provided for a system of second-ballot withdrawals designed to provide the strongest possible opposition to the candidate of the Right. In 1967 and 1973, this agreement paid great dividends in seats won, but in 1978, the parties found themselves unable to reach agreement on a renewed program. This failure served notice that the Left would be unable to form a stable governing coalition. While it gained about 25 seats in the National Assembly, it fell far short of the victory predicted in early polls.

The third problem of unity, that of assembling a stable electoral majority, emerges in Frear's presentation of the national elections of the Fifth Republic. Two important ideas take shape in this discussion. The first is the increasing willingness of voters on the Left to support whichever candidate survives at the second ballot. The Communist electorate has always been reasonably well-disciplined in this regard, but now the Socialist voters seem to be supporting Communist candidates with nearly equal ease. This opening to the left weakens the possibility of any new development in the center.

The second idea is that the Right can no longer trade on the strength of "heroic gaullism" and the related threat of a totalitarian takeover which DeGaulle often intimated would follow a gaullist defeat. Chaban-Delmas attempted to win the presidency in 1974 with such an appeal, and failed miserably. The continued success of the rightist majority since then is perhaps the strongest sign of the continued good prospects for unity in the Fifth Republic.

On the evaluative side, I felt that there were a number of important omissions. The reasons for DeGaulle's dismissal of Pompidou as prime minister in 1969 remain unrevealed in this book. The contents and significance of the Common Program were given very short shrift. The absence of a strong non-Marxist Left in France is a phenomenon which is not discussed. Most seriously, the lingering question of what happens if a president loses his majority in the National Assembly did not receive sufficient attention, in my view.

I found Frear's prose style to be generally excellent. In a few places, the reader is simply overwhelmed with detail (e.g., pp. 175 and 233). However, considering the sort of book this is, the author has given us a highly readable work. His presentation remains interesting throughout.

Organizationally, the book is straightforward and well done. I would propose two changes, however. The chapter on electoral behavior is weak and should be ignored. In its place, I would have preferred to see a chapter tracing the succession of governments in the Fifth Republic.

On the whole, my reaction to this book is highly positive. Frears' portrayal of the major actors on the French political scene displays a considerable sensitivity for the motives which underlie their behavior. And I liked particularly the concise summaries of the reasons why past events retain importance in the present context.

The book will be used to its best advantage by those who seek to integrate their knowledge of the politics of the Fifth Republic. The advanced scholar will find it less useful. No single topic receives more than a few pages' attention. Nor is there any attempt to set the discussion in an overarching theoretical context. However, Frears does present a compendium of data which are current through the municipal elections of 1977. There is an adequate index and an excellent bibliography. The work deserves to be read by anyone with any serious interest in the field.

BRUCE A. CAMPBELL

University of Georgia

The Army in Victorian Society. By Gwyn Harries-Jenkins. (London: Routledge and Kegan Paul; Toronto and Buffalo: University of Toronto Press, 1977. Pp. xi + 320. $12.50.)

A sociologist at the University of Hull and chairman of the British Inter-University Seminar on Armed Forces and Society, Gwyn Harries-Jenkins has undertaking the demanding task of analyzing six decades of civil-military relations under the reign of Queen Victoria. Even though the focus of this book is the nineteenth-century United Kingdom, the author does not limit his data to the British army but includes references primarily to Prussia (later, the German Reich) and secondarily to France and to the United States. His study, which draws heavily from the debates of the Victorian House of Commons and Lords and from the parliamentary papers of the period as well as from memoirs, autobiographies, and contemporary secondary sources, lucidly dissects the pattern of linkages between the corps of active duty and reserve officers in the Victorian army and the landed gentry and nobility of what he terms the "parent society." At least until the formal abolition of the purchase and sale of commissions in the army in 1871, the relationship between the army and the British polity resembled the aristocratic model so ably sketched out by Samuel P. Huntington in the beginning of his classic *Soldier and the State: The Theory and Politics of Civil-Military Relations* (1957), that is, the actors in both the civil and military sectors shared the same ethos and socialization patterns, and they moved effortlessly between the two worlds. Harries-Jenkins aptly employs the term "military squires" (p. 262) to emphasize the interchangeability of the two roles.

Although he does not refer to Huntington's seminal study, he nevertheless demonstrates the degree of access which the Victorian officer corps had to the Palace of Westminster as elected MPs or hereditary peers in the House of Lords, especially until the last quarter of the nineteenth century. This easily secured corridor to power in the parliamentary system tended to mute or reduce the levels of tension between the army and the polity that might have existed had the two been hermetically sealed enclaves, and the army needed no other societal groups to act as its brokers or intermediaries in the political process for the greater part of the nineteenth century. Not only was its power base secure on the Thames but also, through the institution of the Lords-Lieutenant, it was involved in subnational or regional administration in Britain for the bulk of the Victorian era. Such administrative tasks were part of the civic culture of the landed gentry and nobility of that era, an era which glorified the diffuse rather than the specific role, the generalist rather than the specialist, and the amateur rather than the professional or technocrat. Not unexpectedly, family ties and the classical mode of education at the famous (Clarendon) public schools, such as Eton and Harrow, set the tone for the civic culture of the "parent society" and the subgroup of the reserve and active duty officer corps.

The malaise of the army was visible to the British public only at certain unfortunate defeats, such as those sustained in the Crimean War, and the first and second South African Wars, when its opponents were first-rate European powers or were equipped with modern armaments and enjoyed considerable mobility (as did their Afrikaner foes). The faults of the army, asserts the author, were essentially those of the society from which it was drawn owing to the interpenetration of political and military elites. In particular, the army, even after the abolition of the pubchase system for commissions, recruited its officer corps from a very narrow stratum which effectively bolted the doors against the rising industrial and commercial middle classes (which became astringent critics of the army), and it also did not address itself adequately to the question of how such an army could improve its claim to professionalism through a system of advanced military schools, general staff organization, and loyalties that transcended the charmed world of the regiment. Harries-Jenkins draws upon the works of Lasswell, Bagehot, Weber, and Janowitz to explain the nature of that malaise, and he adapts Janowitz's notion of the constabulary concept (developed in Morris Janowitz's *The Professional Soldier: A Social and Political Portrait* [1960]) to one which he terms a guardian role or force (on pp. 176–77, 180–82, 201, 206, 215, 252, 276, and 280), one which suited a land force of an insular power which relied on the royal navy for the defense of the homeland against external foes.

RICHARD DALE

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Mexico in Crisis. By Judith Adler Hellman. (New York: Holmes and Meier Publishers, 1978. Pp. vi + 229. $15.75, cloth; $7.95, paper.)

This is a book which focuses upon Mexican political life in the twentieth century. Its analytical categories are drawn principally from the language of "class struggle." It emphasizes discrepancies between the sweeping reform goals promised at the time of Mexico's Revolution of 1910–1917 and the public policies initiated by political leaders in the name of the revolution during subsequent decades. This general focus upon the dichotomy between promises and practice leads Judith Hellman to note specifically striking contrasts between wealth and poverty, technological development and technological backwardness, promised elections and existing practices of co-optation/repression and finally, highly vaunted political stability and unmistakable signs of political unrest.

Hellman commits herself from the outset (pp. iii–iv) to provide answers to a number of questions that have been voiced with increasing frequency by observers of the Mexican scene:

> What has happened here? Was the Mexican Revolution betrayed? Why is it there are more landless peasants in Mexico today than before

> the Revolution? . . . How does "democracy" function in Mexico? How does the dominant Party maintain itself in power? What is the *meaning* of elections in a one-party system? How do various interest groups make their demands known to the country's leaders? And, finally, how can a ruling party institutionalize a revolution?

All these questions are basically answered in the first two chapters which review Mexican history from 1910 to 1940 with information drawn almost entirely from well-known secondary sources. Hellman's interpretation of the material is familiar and is basic to the orientation of discussion throughout the remaining two-thirds of the volume.

According to the author, "what happened" was that a new class, bourgeois in character, emerged from the revolution of 1910–1917 to grasp control in the name of revolutionary symbols. It was recruited in part from the pre-revolutionary elite and in part from the ranks of the revolution's soldiers. Most leading soldier-politicians who survived the righting identified with the new class, the "national bourgeoisie," rather than with workers and peasnats to whom these leaders had made sweeping promises.

Was the revolution betrayed? Hellman's answer is affirmative and is related directly to her conception of the new ruling class, the "national bourgeoisie." Similarly, the nature of class rule is clearly Hellman's key to how the remaining questions should be answered. "Democracy" does not function in the sense of competitive elections. The dominant party, which is the instrument of the ruling class maintains itself in power by manipulating the old symbols of the revolution's idealism, by subterfuge and by violence, if need be. Elections in the "one-party system" do not mean open, competitive contests for control of government. Only those interest groups tied in with the inner circle of the "national bourgeoisie" "make their demands known to the country's leaders" with substantial effect. "There are more landless peasants in Mexico today than before the revolution" because the interests of the ruling class have demanded land concentration rather than distribution. Finally, "how can a ruling party institutionalize a revolution?" The answer here is less direct because the question is ambiguous, as is the author's treatment of the ruling party, the PRI. The author's interpretation of the role of the ruling class throughout leaves no other role for the party but that of an instrument of its class. The party did not "institutionalize a revolution." In Hellman's view, it was the "national bourgeoise" who appropriated the symbolism of the revolution and created the instruments for control, and one such instrument has been the party structure and banner.

The remainder of *Mexico in Crisis* addresses further ramifications of class rule by the "national bourgeoisie." Accordingly, there is extensive treatment of the patterns and strategies of co-optation, levels of repression, the violence and tragedy accompanying the student protests of 1968 and 1971, the grinding poverty of Mexico's masses—urban and rural—and, finally, speculation on possible alternatives to existing rule—military rebellion or popular revolution.

These are the stronger aspects of Hellman's work. Its weakness lies mainly in the striking scarcity of recent material. Especially noticeable is the reliance upon secondary sources published in 1972 or earlier. The consequences are detrimental. Chapter 3 presents Mexico's "miracle" of economic development as tarnished only by abysmal mass poverty. But the so-called "miracle" itself has become questionable, and this is never clearly expounded nor documented from available primary sources. At the end of the final chapter Hellman mentions such matters as balance of payments deficits, weak internal revenue policy, foreign and domestic borrowing, spiraling inflation, devaluation of the peso, and pressure of population growth. But these serious matters are merely catalogued as ills resulting from bourgeois rule with no concrete information on their magnitude. Similarly, new petroleum sources, Third World relations (especially with OPEC nations), relations with Cuba, and dashed hopes for more profitable ties with the Latin American Free Trade Association are passed by with scarcely a glance. These would not necessarily be problems if the title reflected the thrust of the subject matter in the direction of exploitation and repression, but they are crippling deficiencies in a volume published at the end of the 1970s for the purpose of assessing "Mexico in crisis" as it enters the 1980s.

L. VINCENT PADGETT

San Diego State University

Revolutionary Ideology and Chinese Reality: Dissonance under Mao. By Paul J. Hiniker. (Beverly Hills, Calif.: Sage Publications, 1977. Pp. 320. $14.00, cloth; $6.95, paper.)

Of the numerous studies that have appeared on the Cultural Revolution in the People's Republic of China, Paul Hiniker's is probably the first—certainly the most systematic to date–to apply the theory of cognitive dissonance in an attempt to explain the motivations and origins of that historical upheaval in contemporary Chinese history. The story Hiniker seeks to explain is the link between the Great Leap debacles of 1958 and the Cultural Revolution of the late 1960s. In spinning his tales, Hiniker brings to bear psychological insights and a lot of the quantitative apparatus borrowed from sociology, and yet his meticulous presentation of the massive factual evidence should satisfy historians of the most traditional kind.

The Great Leap had been billed the most advanced model of Communist development predicated upon Marxism-Leninism and the thought of Mao Tse-tung. Since Mao staked his reputation on it, the Great Leap failures cast aspersions on his ability to lead and tarnished the name of his brand of ideology. The revolts within the party's highest councils, as most brutally led by P'eng Te-huai at the Lushan Conference (1959), must be muted if Mao's leadership was to be maintained. In Hiniker's terms, the "cognitive dissonances" contained the seeds of "ideological polarization," which culminated in the Cultural Revolution. The means employed by Mao and the committed Maoists was "ideological proselytizing," aimed at reducing dissonances both in and outside the party. The many campaigns the Maoists launched in the aftermath of the Great Leap, including the indoctrination of the military and the middle-level cadres, the Socialist Education with its broad sweeps, the rectification of the bureaucrats, and the saturation of the mass media with Maoist values calculated to create the "socialist man," should all be viewed as part of the same Maoist proselytizing efforts.

The events had a logical way of building their own momentum. With each failure to contain the dissonances, the stage was set for escalation to a higher level of real or potential conflict. The Cultural Revolution was therefore the logical extension of this deepening process of ideological polarization toward the final showdown between the Maoists and the others who either were not brought in line or refused to recant. The latter included Liu Shao-ch'i, P'eng Chen, Ch'en Yun, Teng Hsiao-p'ing, etc., while figures such as Chou En-lai and Chu Teh intervened between this group and the Maoist group comprising Mao himself, Lin Piao, Chiang Ch'ing, K'ang Sheng, and Ch'en Po-ta, among others.

The theory of ideological dissonance requires the establishment of varying degrees of dissonance before discussions of "dissonance reduction" can gain meaning. By the same token, it is necessary to establish varying degrees of ideological commitment as well as uncommitment to the Maoist line. Hiniker did this by reference to the amount of published support (or detractions) from Mao's thought, and established a "distribution of commitment" to Mao's thought at various times (e.g., pp. 144–45). The difficulty with this quantified measure is that silence, which may in fact indicate either way, would automatically be taken as "uncommitment." Moreover, published verbal support may be misleading, since party leaders who publicly supported Mao's line might or might not be in fact more committed than those others who remained silent. This is a quantification problem to which the author has yet to address himself methodologically.

Quite plausibly, Hiniker offers the thesis of ideological polarization as an alternative to six other explanations on the origins of the Cultural Revolution, including the most common "power struggle" theory. While I am in general sympathy with Hiniker's arguments, I find it difficult to explain the vicissitudes of the Cultural Revolution purely from the perspective of dissonance reduction totally devoid of struggles for power, as Hiniker expounded. Persuasion (or, in Hiniker's term, "proselytizing") was never isolated from coercion, and there can be no coercion without the use of power. In a badly divided situation, one cannot use power without first having attained or regained power, if only as a means to complete the dissonance-reduction exercise. Besides, persuasion can never be conducted in a vacuum; it is always directed at certain individuals or groups, for the benefit of certain others. To the extent Mao was playing various groups against one another–the CCP, the PLA, the state apparatchiks, the Red Guards, etc., and the sub-groups in each of these–in his ultimate efforts at dissonance reduction and consensus restoration, it was at once an ideological campaign and an exercise supreme in power struggle, dislodging certain groups from power and catapulting certain others to power, invariably according to their ideological distances from Mao.

As a whole, this is a fascinating book for the social scientists with an interest in China, especially those who attempt to go beyond the traditional "China watching" techniques and employ borrowed conceptual arsenals so abundantly available from the social sciences. No fact is presented here without being related to a body of coherent postulates, as Hiniker meticulously develops a theory of human motivation for the case under study. In subject matter, the book may appear to be another such study on

China's Cultural Revolution. In methodological outlook, however, it is a total departure from the habitual descriptive studies that have pervaded the China field, most of them rich in fact but poor in theory. The book may be subject to criticisms from traditionalists with no appreciation for social science research for its jargon and neologisms which do seem to impede smooth reading at times. But, I think it is worth the effort to weld social science and China studies together.

JAMES C. HSIUNG

New York University

The Soviet Union and Social Science Theory. By Jerry F. Hough. (Cambridge, Mass.: Harvard University Press, 1977. Pp. xiv + 275. $16.50.)

Although Jerry Hough discusses only one aspect of the relationship of "social science theory" to the Soviet system, his forceful argument lends unusual coherence to this collection of articles. Hough's principal target is neither social science in general nor sovietology in particular; instead, he takes aim at a small group including David Easton, Austin Ranney, Gabriel Almond (and his collaborator G. Bingham Powell), and Robert Dahl (with his disciple Nelson Polsby). In fact, Hough is mainly concerned with criticizing American pluralist systems theory. Whether or not the authors he considers are really "the very best comparative theorists in the political science of their generation" (p. 180), they unquestionably have been the most influential advocates of this particular branch of theory. All derive their models primarily from American experience. Consequently, when Hough criticizes them he is striking at the heart of what he considers to be the principal deficiency in scholarship on the USSR, namely, biased standards of comparison. Similarly, he concentrates on Zbigniew Brzezinski and Alfred E. Meyer because among sovietologists they have produced the most extended comparisons of the Soviet and the American systems. Hough's objective is to redress the balance by showing how similar the two systems really are, or at least how inadequate our conceptual frameworks for distinguishing between them have been.

Two ways were open to Hough to reach this objective: to demonstrate that the pluralist model is inappropriate for both the American and the Soviet system, or that it is suitable for both. At times (in articles, let us note, prepared over a period of eight years) he seems to hesitate between the alternatives. A whole chapter is devoted to criticizing the Dahl school's pluralist critique of the C. Wright Mills-Floyd Hunter theory of an American elite. Hough has little difficulty showing that the Dahl conditions for elite decision making are so stringent that no historical political system could meet them. Hough is much more concerned, however, to demonstrate that, far from being useless, the pluralist model should be used to analyze both systems. To make this point, he seems to consider it necessary to attack in detail the casual remarks rejecting the Soviet system made by the pluralist theorists. I think that anyone who has known these scholars is aware how little any of them pretended to knowledge about the Soviet system going beyond the conventional wisdom of their time; nor, in truth, were they much concerned to pursue the topic.

Hough's more serious endeavor is to define the pluralist model so restrictively that he can plausibly argue (to scholars really acquainted with the USSR) that the model applies there. To accomplish this, he argues that "the major American pluralist scholars of the 1950s and 1960s described both the American political system and the normatively good political system in terms that deemphasized the importance of majority rule, of confrontation politics, and even, at times, of elections" (p. 9). I do not doubt that it is possible to derive such an impoverished model from early theoretical formulations, although scholars like Gabriel Almond long ago moved on to richer historical perspectives. Nevertheless, the implications of Hough's preference are disturbing. Having reduced the pluralist model to such minimal rules that virtually any political system can qualify, he proceeds to posit distinguishing characteristics of an equally reductionist nature. His main criteria are outputs, conceived almost exclusively as material satisfactions. His emphasis on the relative equality of Soviet (compared to American) material incomes (including property income as well as wages) may be factually correct; but concern for material equality does less than justice even to the Soviet regime's recent progress in such an intangible matter as security for the average citizen. Probably no measure of the 1940s was hated and feared by ordinary people as much as the draconic penalties for tardiness or absence at work. Hough's casual reference to "moralizing about the past, especially the Stalin era" (p. 26) appears to me to suggest a certain insensitivity to this kind of output.

Hough's restricted pluralist model is notably insensitive to cultural identity. He accuses his fellow specialists of sheltering themselves in a

"Soviet studies ghetto." One might well counter that a good many of these specialists have been experimenting with a range of theoretical approaches beyond what Hough has dreamed of in his pluralist enclave. Such experiments include sociolinguistics, myth-symbol analysis derived from anthropologists like Claude Lévi-Strauss, and the stress on legitimization in systems theorists like Talcott Parsons, and, indeed, certain adaptations of Marxism. Moreover, such approaches suggest that a far broader comparison than Anglo-American experience is needed to evaluate the Soviet system. Hough speculates rather cryptically that defective comparisons derive in part from the fact that "a great many Soviet specialists have come from a Russian or East European background—some from families that fled the Communists, others from families which left during the pogroms at the turn of the century—and this background may have made them acutely aware of certain types of discrimination or oppression and may have led them to make these phenomena a central test of the overall nature of the system" (p. 223). I incline to the contrary view, that the varied backgrounds of Soviet specialists have made them peculiarly receptive to European theoretical and normative positions, even though I admit that these specialists are remiss in articulating their concepts.

Early in his book Hough remarks that the Soviet system "remains deeply rooted in Russian nationalism" (p. ix). Afterwards he has little to say about the anachronism of a Russian empire based on physical domination of other peoples and nothing at all about the suppression of religion. In fact, the only identity situation which he explores in depth is that of women in the USSR. Here he misses another opportunity to give the Soviet regime credit for a nonmaterial value achievement—the modest degree of liberation of Asian women from extreme Moslem restrictions. In the same context his narrow focus makes him disregard one of the most critical policy choices confronting the Soviet regime: how to handle the immense disparity in demographic growth between European and Asian populations. Even in terms of his own model, however, it is hard to see how Hough could assert that "no such sense [or women's grievance] emerges from the press, except on the issue of husbands' sharing household chores" (p. 150) when in fact quotations like the following are available even in translation:

> Only 15 percent of all medical personnel are men, but they include 50 percent of all chief physicians and executives of medical institutions. In the overwhelming majority of instances, men head departments, enterprises, and administrative agencies. Why is this so? . . . This applies no less to many types of highly-paid mechanized labor, in which men firmly hold the positions they achieved a long time ago (*Literaturnaya Gazeta*, 16 April 1969).

The constricting nature of Hough's model is all the more regrettable, for his rejection of extreme ideological interpretations makes him commendably sensitive to nuances in regime concern for deviation. In what I regard as the best part of the book, he points out that the Brezhnev regime, and perhaps even Stalin's, reacted far more drastically against overt challenge than against obscure speculation about fundamental prescriptions. In other words, social control rather than ideological blueprints has been the overriding concern. This insight, incorporated in models which permitted sensitivity to cultural trends like the growing emphasis on Russian history and folklore, would have enabled Hough to envisage alternative Soviet futures in ways far more relevant to the main streams of "social theory" and social policy than the somewhat passé U.S.-USSR comparison he belabors.

JOHN A. ARMSTRONG

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The Foreign Trade of China: Policy, Law, and Practice. By Gene T. Hsiao. (Berkeley: University of California Press, 1977. Pp. xxi + 291. $22.50.)

Gene T. Hsiao has written extensively about the foreign trade of the People's Republic of China and also about Sino-American detente, working from materials in Japanese as well as Chinese and English. This volume brings together much of this work. The main contribution of this book lies in its explanation of China's foreign trade organization and procedures, and the presentation of the texts of various documents in 20 appendices. Hsiao has made his own translations in a number of cases, and his detailed work, mainly in Chinese-language sources, provides readers with authentic material not readily available together elsewhere.

A particularly strong chapter deals with China's internal organization related to foreign trade. Here is basic information for anyone concerned with China trade on the Ministry of Foreign Trade, on Customs organization and procedures, on the various national foreign trade corporations that handle actual foreign transactions, on the China Council for the Promotion of International Trade, and the Kwangchow (Canton) Trade Fair, the semi-annual meeting of China's trade representatives with would-be traders from abroad.

The appended documents include China's 1975 constitution (superseded in 1978 but of continuing historical interest); trade treaties with the USSR (1958) and several other countries; China's 1964 law on travel, residence, etc. by foreigners; the 1963 law for controlling trademarks; two 1972 contracts with RCA Global Communications; as well as several types of pro forma contracts and documents on general conditions for delivery, on arbitration and on insurance. Hsiao differentiates among three types of foreign countries in Chinese trade: socialist countries, non-socialist countries with which China has full diplomatic relations, and other countries. China trades with countries in all three groups, but the procedures and agreement provisions differ mainly by group. Treaties and agreements carry more significance than usual in trade with China, not only in cases where diplomatic relations are lacking, but everywhere, because China's side of all trade is handled by state organs and because China does not subscribe to many of the international agreements and conventions that govern the bulk of world trade.

One striking feature of China's trade treaties is their heavy reliance on most-favored-nation treatment. MFN clauses, which a century ago featured Western exploitation of a weak China are today one of China's means of agreeing to obtain, as well as give, the most favorable treatment currently being extended with respect to tariffs, customs matters, transit, shipping and other aspects of trade. Another feature is the lopsidedness of China's detailed trade contracts. China has succeeded in getting terms that protect China, whether as seller or buyer, better than the trading partner. This discrimination, the reverse of what China suffered under the "treaty system" following 1842, is reminiscent of China's privileged position as the center of the ancient tribute system.

In addition to these technical sections, the book contains a summary of China's foreign trade experience and policy since 1949, a discussion of the ups and downs of trade with Japan, China's leading trade partner, and very brief introductory and concluding chapters. This material is potentially the most valuable in the book for some readers with limited interest in technical details. The chapter on Sino-Japanese trade is such as to be useful for readers not familiar with this subject (but why did Hsiao omit the important Yoshida Letter from his appendix?). I found the general chapter on China's trade disappointing, even though it goes well beyond its title, "Foreign Trade Policy Objectives." Neither of these survey-type chapters offers significant new insights or otherwise goes beyond what is easily available elsewhere.

My strongest criticism of this volume is that Hsiao does not use his detailed material to maximum effect for readers who are interested in general principles and trends, or relations between the details reported and other phenomena in China, in Asia, or in the world at large. The book does present some generalizations and conclusions, but they are often disappointing. Some are too brief, broad and obvious to be helpful. Some are inadequately related to the detailed material presented. So I must rate Hsiao as stronger at legal researching than at economic or political analysis.

WARREN S. HUNSBERGER

American University

Political Power and Communications in Indonesia. Edited by Karl D. Jackson and Lucian W. Pye. (Berkeley: University of California Press, 1978. Pp. xxi + 424. $18.50.)

This book consists of thirteen chapters by ten authors, none of whom, inexplicably, is co-editor Pye. The tone and focus is largely set by Karl Jackson who contributes four of the essays.

The core theme is contemporary Indonesia as a bureaucratic polity, defined by Jackson, to paraphrase slightly, as a political system in which power and national decision making are shared almost exclusively by the employees of the state, and especially by the topmost levels of the officer corps and civilian bureaucracy, including the technocrats. As such, the exciting political pluralism of an earlier Indonesia has been replaced by a much more staid animal. For Jackson, the essentials of the system have been in place at least since the demise of parliamentary democracy in 1957.

Not that this bureaucratic polity has been static. Under Shuarto, power has been tightly concentrated. Ulf Sundhaussen describes the process within the armed forces by which power once diffused in separate army divisions, officer cliques and the various services has been gathered into "a new peak in terms of cohesiveness, discipline, and hierarchy" (p. 78). Further, Sundhaussen asserts, national decision making has been drawn into the hands of one person, Suharto, so that the regime has come to resemble such personal ones as those of Ataturk and Nasser "in which army leaders in the role of president use the military for the purpose of policy implementation but successfully deny it a role in policy formulation" (p. 78).

Donald Emmerson delineates the process of power concentration within the civilian bureaucracy, along with the concomitant insulation from extra-bureaucratic influences. The present end result is "monoloyalty" of bureaucrats to the government, the closing of upward communications between governed and governors, and the assertion of a poorly informed, if at times well-intentioned, commandism over the populace. Another result of the shut-off of "inputs" into the system may well be, as Jackson states and others imply, that the total power available to the rulers (that is, the ability to move rather than merely sit on Indonesia) has not been significantly increased. Shades of Soviet agriculture!

Several authors examine the output side of the polity. Certain "achievements" have been wrought in the economy, as Bruce Glassburner indicates. On the other hand, as he and others show, many fundamental problems are left unresolved and in most or all cases with no solutions in sight: for example, inadequate food production, Java's population growth, grossly unequal distribution of societal "rewards," and a partly unabsorbed labor force growing by 1¼ million persons per year. Gary Hansen poignantly portrays the peasants (stupid in the eyes of many of the elite) being buffeted by the wildly fluctuating rice production policies emanating from the center.

On the mass media, there is a sketchy and unsatisfactory attempt at an overall perspective by Astrid Susanto, and Nono Makarim's sensitive insider's view of the life of a newspaper editor in today's Jakarta.

Jackson asserts that "the Indonesian bureaucratic polity is stable but weak, easy to rule but difficult to modernize" (p. xx). But do the rulers (or does the ruler) face no clouds on the political horizon? Will the Indonesian people continue to be herded uncomplainingly into a less than rosy future? William Liddle argues for the continued and strong presence within Indonesia of clashing and irreconcilable concepts of the good life. Where Suharto and his central Javanese *abangan* inner clique favor hierarchy and deference, there are still powerful yearnings for an egalitarian and participatory society. There are also the desires for regional autonomy and an Islamic State. On the latter, however, Allan Samson in his study of current Indonesian Islam concludes that "religion appears to be as much a legitimizing as a motivating force" (p. 213), and that the general elections held by Suharto have shown Islam to be a far less powerful political magnet than was previously believed—and feared.

All the authors would agree that for the foreseeable future civilians are too weak to replace or civilianize the present polity. Change, then, can come only from within the officer corps over which, at present, Suharto has tight control. However, unlike the military in some other countries, Indonesia's officers have not even begun to institutionalize presidential succession or a circulating executive. Moreover, the younger officers have entirely different life, political and educational experiences from their seniors, those of the Japanese occupation and anti-Dutch revolution. The next generation *will* be different. No author is rash enough to prophesy in what way, although more than one dares to speculate. A resurgence of Javanese populism, perhaps?

Major omissions of coverage prevent this book from being a broad introduction to Indonesian politics. Little or nothing is said about the students or young educated people in general, about the Outer Islanders, the peasants (other than as objects of inept elite economic manipulation), or even the highest civil servants including the technocrats. Government, politics and economics below the national level are similarly slighted.

More than one contribution is faulty, insubstantial or out of place. To be fair, however, *Political Power and Communications in Indonesia* deserves two hearty cheers. The authors as a group have postulated, explored and begun to argue about a model of the Indonesian political system that others may accept, modify or reject, but not ignore.

DONALD HINDLEY

Brandeis University

Social Change in Romania, 1860–1940: A Debate on Development in a European Nation. Edited by Kenneth Jowitt. (Berkeley: University of California, Institute of International Studies, 1978. Pp. xi + 207. $4.50, paper.)

This volume is a collection of essays written for a conference on social change in Romania: "The Socio-Cultural Bases of National Dependency in Peasant Societies" (Kenneth Jowitt): "Neo-liberal and Social Democratic Theories of Development: The Zeletin-Voinea Debate Concerning Romania's Prospects in the 1920's and Its Contemporary Significance" (Daniel Chirot); "Notes on the Romanian Debate on Sheltered Industrialization: 1860–1906" (John Michael Montias); "Modernization and Decay in Historical Perspective: The Case of Romania" (Andrew C. Janos); "Reflections on Mihail Manoilescu and the Political Consequences of Delayed-Dependent Development on the Periphery of Western Europe" (Philippe C. Schmitter); Gîndirea: Nationalism in a Spiritual Guise" (Keith Hitchins), and "Variable Sociopolitical Functions of Aesthetic Doctrine: Lovinescu vs. Western Aestheticism" (Virgil Nemioanu).

The subject matter covered in the book is most interesting, and the various contributions bring to light much material which has not been adequately studied before. Briefly put, the authors attempt to show that the political, socioeconomic and cultural debate on strategies of development had great significance in Romanian public life during the roughly 80 years which separated national independence for Romania and the full-blown dictatorship with strong fascist overtones which was instituted in 1940. Furthermore, many of the contributors examine the similarities between the Romanian development debate of this period and the contemporary debate on the same topic in developing countries. But perhaps the most interesting part of the book deals with the extent to which several Romanian scholars (and even some political leaders) produced works of high quality in this debate in the period between the two world wars—works which already then discussed many of the important issues in the *contemporary* scholarly debate on development.

For Romanian scholars and public figures, the problems of social development always revolved around the need for *models* and the choice between such models. Should one accept the approaches undertaken by the industrialized West as a guideline for *Romanian* development, or were there specific local and national conditions which would require another, special road to "modernity?" Could rapid development be achieved through a free market or through mercantilism? What were to be the *political* strategies—capitalism, socialism, or fascism? A major theme running through this interesting debate dealt with the effects of modernization and development upon the Romanian "soul"—the culture of its population and the ages-old traditions of the peasantry in that country. This theme was equally important for socialists, mercantilists, and fascists, although the programs they advocated were rather different in practical terms.

Social Change in Romania represents a considerable addition to the scholarly literature on development; I would nevertheless have wished that there could have been room for a concluding chapter which would have synthesized its variegated contents. Interesting comparisons could also be made with the present-day attempts at a "Romanian road" to development and the quest for autonomy from strong outside forces, this time the Soviet Union and *its* development model. This is not a major objection, however, and I find *Social Change in Romania, 1860–1940* to be both stimulating reading and a contribution to the field of political development.

TROND GILBERG

Pennsylvania State University

New Trends in British Politics: Issues for Research. Edited by Dennis Kavanagh and Richard Rose. (London: Sage Publications, 1977. Pp. 254. $11.95, cloth; $5.75, paper.)

This is a collection of essays by British scholars focusing on areas of potential research in British politics. The contributors are mostly behavioralists, and indeed a theme developed in the first essay by Sam Beer is that the failure of traditional economic and political practices in Great Britain in the last 20 years has created a pressure for new approaches. As Dennis Kavanagh and Richard Rose outline in their survey of British political science, there has been a significant shift in Great Britain towards a more "self-consciously professional and specialized" discipline (p. 21). This has been reflected in the rapid growth of the Political Studies Association, the more frequent application of quantitative techniques and the proliferation of original research in such journals as *Government and Opposition*, the *British Journal of Political Science* and *Policy and Politics.* Apparently, a troubled Britain may stimulate more innovative research than a tranquil Britain ever did. Having observed the course of the behavioral revolution in the U.S., Beer is more cautious. The unbounded aspirations of American behavioralists were not fully realized, he tells us, and the war with the traditionalists has led to an "equilibrium without consensus" (p. 3). As to whether behavioralism will resolve Britain's crisis, Beer only says that he hopes "that the new methods of study may also be of some help in resolving it" (p. 6).

There are two basic themes in this book. The first is that the problems of the seventies have challenged British practices and institutions in ways which are not now well understood. An example discussed by David Coombes is the constitutional ambiguity created by Britain's entry into the Common Market. The Common

Market introduces two hitherto unknown components into British politics: federalism and a written constitution. There is a need to know more about the potential conflict between community and domestic law, the effect of membership on the supremacy of Parliament and on ministerial responsibility, and the status of citizen's rights acquired through EEC membership. Most importantly, no one knows whether the Common Market will have centralizing or decentralizing effects on British government. Richard Rose also deals with this question in his essay on the effects of devolution on the constitution, suggesting that we know very little about the kinds of "between-nation relationships" which will develop in government departments and in political parties after devolution goes into effect. Then, of course, there is the crucial question considered by Dennis Kavanagh and Harris Miller of how these problems have changed the political system. The cross-cutting nature of issues in the seventies has made prominent the various pressures placed upon MPs by their constituency parties, their ministerial aspirations, voters and important interest groups such as big business and the TUC. The idea that these demands are conflicting is novel and forces a closer look at the role of the individual MP.

A second major theme in this book is the need to study more closely the effects of intended and unintended changes in British political institutions. Colin Seymour-Ure considers the changing structure of the British press—the independence of papers from ownership by the parties, the declining number of papers, and the emphasis of the press on nonpolitical events—and explores the significance this has for the way in which political attitudes are determined, how the agenda of politics is controlled and the power that newspapers can exert at both the national and local level. Ken Young looks at the expansion of local government planning and urges future studies to see that local decision making is constrained by uncertainty and the decline of local economies. Wyn Grant looks at the phenomenon of increasing tripartite cooperation between the government, the TUC and the CBI. He suggests that we need to know more about the means which interest groups have of controlling their members, the ways in which we can measure the effectiveness of interest gropus, and the relationship between the media and interest groups. Rudolf Klein explores research on the growth and distribution of government expenditure, and in particular points out the difficulty of pinning down the role of political influences on the economy and government services. Finally, L. J. Sharpe suggests some topics on post-Fulton changes in the civil service, providing the reader with a most useful bibliography and some discussion of the problem of collecting data on this topic.

One is perhaps tempted to criticize this collection for being what it is (an agenda for research) rather than what it is not (results from current research). However, there are some fine essays and excellent bibliographies in this collection. It serves well the purpose for which it was intended and should stimulate some quite interesting research.

BRUCE E. CAIN

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Red Years/Black Years: A Political History of Spanish Anarchism, 1911–1937. By Robert W. Kern. (Philadelphia: Institute for the Study of Human Issues, 1978. Pp. xiv + 335. $19.50, cloth; $7.95, paper.)

We are fortunate that a small flood of books on Spanish anarchism have been made available in the past couple of years, some appearing for the first time in English translation. Most have been issued as part of a resurgent publishing effort from within the anarchist movement. Among the more worthy of the recent issues are Murray Bookchin, *The Spanish Anarchists* (Free Life and Harper); Gaston Leval, *Collectives in the Spanish Revolution* (Freedom); Abel Paz, *Durruto: The People Armed* (Black Rose); José Peirats, *Anarchists in the Spanish Revolution* (Black and Red) and the work at hand. Several more are in the wings, including the anthology *A New World in Our Hearts* (Black Thorn/Cienfuegos) and David Porter's work in progress on Emma Goldman in Spain.

Kern's *Red Years/Black Years* is a very competent and well-researched volume. The book is directed towards a political history of a fundamentally antipolitical movement, and herein rest both its major strength and much of its weakness.

The popular impulse of the Spanish litertarian movement—anarchist and syndicalist—was primarily antipolitical. Particularly among the anarchists, there was a deeply rooted and instinctual horror of the state in all its manifestations, as well as a sincere fear of being tainted or corrupted by participation in what we call politics. Other works on Spanish anarchism have better captured, with greater vitality, this particular passion of the Spanish anarchists, though Kern's treatment provides a very welcome antidote to a romanticized view of the anarchist as fairly exclusively the "primitive rebel." There *were* primitive rebels, to be sure, but Kern tends to see them as flies in the ointment rather than as an important component of the total anarchist configuration in Spain. He tends to regard the heroic rebels as spoilers of a maturation process he posits whereby the FAI (Federación Anarquista Ibérica) might have evolved into a full-fledged, responsible political party, albeit a revolutionary labor party.

Kern's criticisms of the anarchists revolve around the "mistakes" they made which cost them the opportunity to get into power. He even goes so far as to state that only the direct intervention of the Russians and fascists "destroyed the anarchists' bid for power" (p. 246). He knows that the idea of seizing or assuming state power is anathema to the consistent anarchist, and does admit that "a lingering suspicion remains in the minds of many old anarchists that the real cause of defeat was the abandonment of traditional libertarian values" (p. 246). But he seems to give this other view rather short shrift. Kern admirably portrays the political development of a leadership segment within the FAI, and this provides welcome balance to the stress on the primitive rebel. He does not, though, seem to credit sufficiently the possibility that the political calculations and intrigues of these prominent anarchists may not have been representative of the broad base. On the other hand, he suggests that to speak of a movement is at times misleading, when one could more aptly characterize the "movement" as "merely a jumble of groups trying to speak for a tradition of rebellion that was instinctive but long repressed" (p. 87).

In the way of disappointments, the book lacks an empathy for the (perhaps quite naive) exhilirating sense of possibility which blossomed in the minds of the libertarians in Spain; this is not to suggest that one need be sympathetic to the anarchists, but this lack suggests a less than complete comprehension as to what the anarchist mind is all about. It also seems to emphasize inadequately the *realpolitik* perspective on big power machinations so thoroughly explored elsewhere by Broué and Temime. For instance, Kern apparently assumes that the reader is familiar with the Non-Intervention Committee, and provides little discussion of the crucial impact the denial of substantive outside support had upon the Spanish movement besides the obvious over-reliance on Soviet assistance. One other minor point: it is really incorrect to see "propaganda of the deed" as simply a "euphemism for assassination and terrorism" (p. 21).

These criticisms aside, Kern's book is a useful and significant contribution to the study of Spanish anarchism. Its fairly distinctive emphasis on the political development of the movement is of great value and, as I have indicated, offers a welcome balance to some of the prevailing interpretations of the anarchist phenomenon. It is certain to spark some welcome controversy.

WILLIAM G. NOWLIN, JR.

University of Lowell

Korea: A Divided Nation. Edited by Se-Jin Kim and Chang-hyun Cho. (Silver Spring, Md.: Research Institute on Korean Affairs, 1976. Pp. vii + 330. Price not given.)

This book, designed as a test for undergraduate study of Korea in the United States, is an anthology contributed by eleven Korean scholars educated in the United States. They are seven political scientists, three economists, and one jurist.

Two Koreas could provide a necessary condition for a comparative study of developing countries existing in two ideologies, communism and "democracy." This volume achieves that end. Why did such an old nation of one race, one language and one history divide into two nations? Who divided Korea? These are old questions answered in Se-Jin Kim's introduction and Samuel S. Kim's "Developmental Problems of Korean Nationalism" (in part 1).

The present status of South Korean politics and economics is discussed in the articles (part 2): Se-Jin Kim, "The Government and Power Structure in South Korea"; Young Whan Kihl, "Leadership and Opposition Role Perception among Party Elites"; Chang Hyun Cho, "Bureaucracy and Local Government in South Korea"; and Seung Hee Kim, "Economic Development of South Korea."

Kihl's article is the only one using a field survey of party elites in South Korea. His findings should be tested against data from other developing nations. They are that

> (1) the party elites in Korea are generally authoritarian in their perception of, and orientation toward, the leadership role, although the ruling party members are more so than the opposition party members; (2) the party elites in Korea are not uniform in their orientation toward, and perception of, the opposition role in political process, although the variation within the opposition party is greater than within the ruling party members; (3) the incidence of the party's elites' role perception

> which is not congruent with the expected pattern is greater among the opposition party members than among the ruling party members; (4) the status of party elites in the party hierarchy has proved to be a discriminating factor of congruence between the perception of leadership and opposition roles; and (5) the regional difference of party activities did prove to be important in affecting the pattern of role orientation of party elites in Korea, for both ruling and opposition party (p. 80).

North Korea's political, economic, and legal structures are discussed in part 4: Chin O. Chung, "The Government and Power Structure in North Korea"; Chang Hyun Cho, "The System of Local Government in North Korea"; Joseph Sang Hoon Chung, "North Korea's Economic Development Strategy and Economic Performance"; and Sung Yoon Cho, "The Judicial System in North Korea."

Chung's propositions from his economic analysis are comprehensive:

> (1) the North Korean leadership is committed to economic development via rapid industrialization, particularly heavy industry; (2) the North Korean model is characterized by a heavy defense burden; (3) North Korea's industrialization was made possible by the infusion of a large labor force and capital. A rapid change in the structure of the economy in favor of industry followed; (4) North Korea remains one of the communist nations' most centralized, socialized, and planned economies generally pursuing the Stalinistic development model; (5) non-pecuniary incentive systems continue to be widely and consistently relied upon as substitutes for material incentives; (6) rapid expansion in investment in human capital, especially in technical education, has been of high priority in order to advance growth-supporting activities; and (7) North Korea's sizeable loss of population due to war and the outlaw of refugees made her a relatively labor scarce economy, a rarity among high-density Asian countries (p. 199).

These propositions are already tested by the author and other contributors to this volume.

Two Koreas are discussed in comparative perspective in part 4: Sung Chul Yang, "Ideology in South and North Korea"; Tae Won Kim, "Two Economic Systems in Comparative Analysis"; and B. C. Koh, "The United Nations and the Politics of Korean Reunification."

Koh's conclusion answers the question: why is Korea still divided and into two Koreas?

> To this observer, the most striking aspect of the 1973 Korean drama in the United Nations was not so much the substance as the manner of its resolution: what eventually emerged was less a compromise by the two Koreas than a compromise for the two Koreas. With sizeable delegations from Seoul and Pyoungyang on the scene, there were abundant opportunities for face-to-face negotiations. Yet direct communication between the two sides was conspicuously absent. That it required the mediation of third parties speaking alien tongues, to hammer out a compromise agreement was indeed a sad commentary on the status of the North-South "dialogue." It also cast a dark shadow over prospects of Korean unity (p. 285).

This personal but evaluative statement of two Koreas rejects the preface's optimism on changing the situation in Korea in 1972 through an epoch-making dialogue for reunification after a quarter-century of adversary relations.

Since the editors assembled articles which were already published in the *Journal of Korean Affairs* and *Korea Journal* and unpublished works without serious editorial leadership, they failed to insure that the topics and areas covered in articles were balanced. Obviously, there has never been any possibility of empirical research on North Korea. However, a wide range of topics and areas could include sociologists and social psychologists using content analysis techniques for more systematic and scientific inquiries on two Koreas' political phenomena. The coherence of an integrated theme is desirable.

Most articles in this volume are more descriptive than analytic. After many descriptive studies, do we expect analytical ones? I should not be too critical of this modestly ambitious volume. I just wish that the best description would lead to the best explanation.

YEARN H. CHOI

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Interest Groups in Norwegian Politics. By Robert B. Kvavik. (Oslo: Universitetsforlaget, 1976. Pp. 206. $11.00, paper.)

This is an extraordinarily useful book for students of interest groups in particular and European and American politics in general. Kvavik argues that the structure, role and behavior of interest groups in Norway are fundamentally different from those in the United States. Political parties are said to pressure interest groups, rather than the reverse; interest groups participate in policy making and implementation, rather than pressuring administrative and political leaders toward some particular option; and groups, in a relative sense, are much more likely to focus their efforts on administrators rather than, as in the American case, on national legislative committees. In a more general theoretical sense, these differences help us to assess the validity of the competitive pluralist and corporate pluralist models of interest group behavior.

This book is important from at least four perspectives. First, the predominance of American social science work has often led to the assumption that American patterns hold in other national settings; competent works, such as this one, are pointing increasingly to the view that the American case may be the exception rather than the rule. Second, the subject of interest groups is important in its own right, for there is probably more bunk written about interest group pluralism than about any other subject. The more systematic studies of this sort we have, the better. Third, there are efforts throughout to compare the Norwegian case with other patterns in other nations; comparativists will find these useful. Finally, for its information and description, this book is helpful. It provides substantial details on the types, activities, and political orientations of Norwegian interest groups; such comprehensive information indicates rather extensive footwork by the author.

This study's principal weakness is methodological. The theoretical perspective and the complex questions it raises about the role of interest groups in society demand innovative questionnaire design, an effort to deal with such questions as systematically as possible, and a familiarity with new techniques and measurements in social science research. Yet, comparison of the brief list of straightforward questionnaire items in Appendix 1 with the extensive theoretical discussion in chapter 1 indicates that interaction between empirical observation and theory is quite limited. Recent developments in social science research should make it less and less necessary to rely on softer evidence; yet, much of this work does exactly that. And, the systematic evidence which is provided is not particularly innovative—given the complexity of the task. None of the more recent innovations in questionnaire construction, index design, or scaling appears here; rather, the questionnaire is limited in content and nominal in data-recording instruments.

Readers interested in European politics, interest groups, pluralism, and policy making will all find this an interesting and useful work; but, from a methodological standpoint, it breaks no new ground and, indeed, backtracks a bit.

ANDREW T. COWART

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Khrushchev: The Years in Power. By Roy A. Medvedev and Zhores A. Medvedev. (New York: Columbia University Press, 1976. Pp. xi + 198. $10.95.)

This short book has a modest aim. It provides a sketch of Khrushchev's years in power as seen by two observers "from within" the Soviet Union for whom those years are "still living history." The authors are twin brothers both prominent in their own right in the Soviet dissident movement: Roy Medvedev, a historian and author of the work on Stalinism, *Let History Judge* (New York: Knopf, 1971) and Zhores Medvedev, the geneticist and author of *The Rise and Fall of T. D. Lysenko* (New York: Columbia, 1969). In this latest book they seek to counterbalance the more or less detached Western analyses of Khrushchev's rule "from outside."

The book is basically a documented analysis of Khrushchev's agricultural policy—seen as the key to his rise and fall—fleshed out with accompanying political commentary which seeks to capture the atmosphere of the Khrushchev period "as it was felt by those living within the Soviet Union" (p. ix). The political analysis contains little specific documentation. It draws not only on the authors' own observations and reflections over the years but also on the views of a number of unnamed Khrushchev-watchers within the party and state *apparat* with whom the Medvedevs have had contact.

The book does not purport to be anything like a biography of Khrushchev or even a history of his rule. Khrushchev's own extensive taped memoirs are not used, since, as Zhores Medvedev explains it, they contain little self-critical analysis of his political and economic errors and little on his agricultural policy, the central theme of the book.

It can be said, in fact, that the book's concentration on Khrushchev's agricultural policy—its initial successes followed by near-disasters—constitutes both its virtue and its defect. This part of the Khrushchev story is well told. The analysis is supported by telling statistics drawn from official Soviet sources themselves. However, the book's thesis that agriculture was the prime cause of Khrushchev's downfall rather than an important contributing factor is not wholly persuasive. Nonetheless, the elements of a more convincing and comprehensive *political* explanation of Khrushchev's decline and fall can be found in the book's political commentary. However, these elements do not coalesce into a cohesive explanation, since they tend to be shunted aside by the book's focus on agricultural policy and economics. However, in their general observations the authors quite correctly see Khrushchev as a radical innovator and reformer at least from the standpoint of the rigid confines of the one-party dictatorship. They recognize that Khrushchev attempted a major redefinition of the ruling party's role not just in agriculture but across the whole spectrum of policy, not excluding the inner structure of the party itself. The authors allow that neither "his economic or administrative blunders" (p. 151) led Khrushchev's adherents to switch loyalties and abandon him. Rather, they say, the switch began with his plan to require in party statutes a regular rotation of leading party functionaries and his reform dividing the party into industrial and agricultural sub-parties. In application, the authors point out, these changes proved to be incompatible with the one-party system and gave birth, however briefly, to something like a nucleus of a two-party system, "though still unified by a common ideology and platform" (p. 155).

However, the Medvedevs do not adequately place the party reforms within the political dialectic of Khrushchev's rulership. These reforms arose logically from and capped all of Khrushchev's earlier reforming and innovating efforts—his de-Stalinizations, decentralizations, his varied attempts at loosening up the Soviet system. These last major reforms simply brought to a head the dilemma of Khrushchev's reformism—in fact, the classic dilemma of all political reformers—namely, how to carry through and consolidate basic reform before all the forces opposing change have time to gather and defeat it. When his party reforms made his purpose unmistakable, the conservative consensus of the party turned decisively against him and he was driven from office. The Medvedevs' citation on unnamed critics within the apparatus expresses the gist of their position very well: "We must return to old reliable forms of secure and stable centralized leadership" (p. 170).

The Medvedev brothers, in short, do not give enough attention to the *political* as against the technical or economic complexity of the task Khrushchev set for himself. As a consequence the authors praise Khrushchev's good intentions but damn him as a bungler. Such a conclusion depreciates Khrushchev's almost Sisyphean labors, the political skill, intelligence and energy with which he pursued his purpose of renovation for 11 years before he finally succumbed to massive odds. He at the least deserves credit for a titanic attempt and a magnificent failure which may still be working a not wholly visible yet deep effect on the Soviet system.

CARL A. LINDEN

George Washington University

Peasants, Politics and Revolution: Pressures toward Political and Social Change in the Third World. By Joel S. Migdal. (Princeton, N.J.: Princeton University Press, 1975. Pp. 301. $15.00.)

All great revolutions pose great intellectual puzzles. The Chinese and Vietnamese revolutions are but the most recent example of that fact. More poignantly than the French, Mexican and Russian revolutions, they raise the question, among others, of the magnitude of the role which the peasantry can play in bringing about rapid, radical institutional changes, and in winning the (literal) battles which invariably accompany this kind of change. The question is indeed an intriguing one when one considers these two Asian revolutions. For peasant revolts—of which there have been many throughout history—have either been bloodily repressed, or have contributed to change only when the peasants were part of a larger (generally unformalized) coalition with various urban groups. That was precisely the case in 1789, 1910 and 1917 (not to mention Bolivia in 1952 and the Cuban Revolution in 1958). But neither in China nor in Vietnam did this occur. Here, there was no one else with whom to form an alliance, except that vanguard of professional revolutionaries which with much justification Lenin had seen as a *sine qua non* to revolution in any halfway modern society.

Migdal's book—which is a most stimulating piece of work—addresses itself ultimately to the problem of the conditions under which peasants commit themselves to long-term revolutionary activities. The book's data base is twofold. Relatively little used is a quantitative analysis (which surely must have taken a great deal of time) of 51 monographs dealing with peasant and village life and its transformation. More frequently, and very convincingly used, are appropriate quotations from the entire range of writings on peasant movements, peasant communities and their linkages and rural revolutions in various places and at various times. In addition, then, to its own theoretical contribution, this book is in essence, an imaginative synthesis and time-ordering of the existing literature. Migdal proposes a "historical stage" model of peasant reaction to incorporation in the world market. In the first stage, the already existing tensions between incipiently poor and incipiently rich in the traditional village are set forth, as well as the mechanisms by which these tensions are contained. The impact on the village of "the spread of world-wide imperialism" is then examined, as it destroys the institutions restraining those who can and want to do so from entering the world market.

As the village becomes more outwardly oriented, it becomes more polarized in wealth and power; the old integrative institutions are sheltered; and the poor are abandoned to their fate precisely when externally originating crises become deeper, and externally originating exactions (e.g., taxes) become less flexible. In this kind of situation, the success of outside revolutionaries—and this is a central point in the latter part of Migdal thesis—is not determined by the greater or lesser attractiveness of their ideology, but by their capacity to build institutions which will solve the mundane, existential problem of peasants in the here-and-now: relieve them of taxes and rent now, not after the revolution; redistribute income and land; offer younger peasants a chance for advancement, etc.

The argument covers a great deal of ground and is vividly set forth—at times a little too flamboyantly, as Migdal himself appears to realize. In the final chapter, he admits that "a shrinking world" is responsible for the peasant's woes—admittedly brought by a capitalist-imperialist system, but probably more basic than the system. Because this is a synthesis of literature (and its coverage of the literature is itself an enormous achievement), the possible weak points in this daring argument are easy to spot. This is good science, but leaves the author vulnerable. First, there is a *deus ex machina* quality to it. If everything hinges on the ability of revolutionaries to offer peasants a useful alternative institutional framework while the revolution is still ongoing, the question naturally arises: how did the revolutionaries ever get as far as to institutionalize themselves? Half the battle seems to be over before the explanation begins.

A second weakness, inherent in any imaginative integration of existing literature and therefore not at all the author's fault, is the variable quality of the evidence available to him linking one part of the argument to another. Mark Selden, Roy Hofheinz and William Hinton for China; and Samuel Popkin, Jeffrey Race and many others for Vietnam, have amply illustrated Migdal's point concerning the immediate, pragmatic services rendered to peasants by successful revolutionaries. Barring any "revisionist" explanation, the point is well established, and the only difficulty is the negative instance: why did it *fail* when it was tried in Latin America? In other words, do two instances prove a point?

More serious is the rather skimpy evidence available to Migdal (and to anyone else who wishes to integrate this literature) for his initial portrait of the village as already containing an incipient class structure, but one constrained by institutional pressures, which limit contact with the outside and preserve approximate equality within the community. The examples cited are intriguing and vivid: but they have to work for an awful lot of peasant history.

But these are minor blemishes, more related to tone of presentation than in substance. There are now appearing a limited number of major works on peasant revolutions—Jeffrey Paige's *Agrarian Revolution* (Berkeley: University of California Press, 1975), with its daring statistical approach being another. Migdal's book is among the very few and select.

HENRY LANDSBERGER

University of North Carolina

British Business and Ghanian Independence. By Josephine F. Milburn. (Hanover, N.H.: University Press of New England, 1977. Pp. x + 156. $12.50.)

Relying primarily on business records and public documents, this book examines the contribution of British business firms in Ghana to economic development and political change in that country. Josephine Milburn makes an attempt to discover to what extent the general concepts "capitalism," "exploitation," and "innovation" characterized various phases of business activity from 1937 until the early 1970s, and, more specifically, what relationships existed between business and government in the process of change, and whether business firms had the capacity to accelerate their contributions to economic and political change.

These are significant questions, and Milburn musters an impressive array of documentary evidence in attempting to answer them. Problems with both theory and data prevent her from arriving at entirely satisfactory answers, however. It is easy for her to substantiate that British firms behaved in a capitalistic manner, but this is hardly surprising. While the terms "exploitation" and "innovation" are defined with reasonable precision, the relationship between them is left somewhat ambiguous. Milburn implies that they are polar opposites, although this opposition is neither inherent in the author's definitions nor supported by the author's data, which clearly indicate that the firms were both "exploitative" and "innovative." Neither the definitions employed nor the data presented are sufficiently precise to provide meaningful measurement of degrees of exploitation or innovation.

Although supplemented by interviews, documentary data are most frequently allowed to stand on their own. Numerous quotations from a wide variety of documents, as well as extensive summaries of many others, provide a wealth of detail, but the information contained therein is not always that which would be most relevant for answering the theoretical questions Milburn poses, questions which call for the measurement of variables.

There are, furthermore, two types of imbalance in the documentary sources which were available to the author. First, within the business community Milburn examines only three firms extensively: United Africa Company, Cadbury Brothers Limited, and John Holt and Company. Since all of these were primarily involved in various aspects of the cocoa trade, this sector of the Ghanian economy, and more specifically these three companies, are the overwhelmingly predominant focus of the book. A number of other firms in the fields of banking and mining, and the supplementary activities of the focal firms in a number of fields are examined—particularly with regard to Africanization of management and problems of labor relations—more or less as an afterthought. Generalizations Milburn puts forward with respect to the major questions raised in the book refer essentially to the three focal companies. While the significance of cocoa for the Ghanaian economy certainly justifies its central position in this analysis, Milburn should have distinguished more carefully between three significant firms and all business.

A second imbalance involves Milburn's more complete access to business documents than to government documents. While the book by no means presents an entirely one-sided business point of view, extensive summaries of and quotations from business documents cause her to view many issues predominantly through business eyes. The reader simply gets to know business actors better than their governmental or political party counterparts.

In spite of these theoretical and data problems, the author arrives at some interesting, if somewhat uncertain and not always adequately substantiated conclusions. Her conclusions regarding "exploitation" and "innovation" have already been noted. Both of these characteristics are shown to have resulted from the pursuit of profit through means including the enhancement of Ghanaian buying power. Milburn also found that business initiated contacts with government, rather than vice versa, but that, once established, these contacts enabled business to exert substantial influence on matters of direct relevance to it. With regard to more general issues of change, especially political change, business followed the lead of government, neither urging it forward nor attempting to hold it back, but helping to implement governmental decisions once they had been made. Whether business in general, and especially the three focal firms, could or should have done more than this Milburn does not really say.

JAMES R. SCARRITT

University of Colorado

The 'Kept Men'? The First Century of Trade Union Representation in the British House of Commons 1874–1975. By William D. Muller. (Atlantic Highlands, N.J.: Humanities Press, 1977. Pp. xx + 283. $21.50.)

The first part of William Muller's analysis of trade union-sponsored members of Parliament deals with the varying characteristics of union-sponsored Labour members between 1874 and 1975. Through analysis of surveys made during the 1960s he indicates the general characteristics of these members today and how their qualities have changed within three general periods of trade union political sponsorsip. Finally he evaluates the effective transference of union views to party policy, and the effect of these sponsored members of Parliament on the policies of the cabinet and the government in several areas from 1900 to the 1970s. He emphasizes two policy areas—defense in 1960–61 and prices and incomes from 1964 to 1970. The work is well done when considered within the limits defined by the author, yet because of the diversity of views among the unions on general policies, it may not be possible to conclude in general terms about the effect of union views in these areas.

In a review of the book, a union leader does not dispute Muller's facts, but does question his interpretations and conclusions (see *The Miner,* January-February, 1978). It is difficult in one volume to cover accurately 100 years of union history and the differences between the unions. Perhaps it may be impossible to come to general conclusions that apply to all the unions and their sponsored MPs.

Some points that should be corrected include specific references to each union. I will use the Miners' Union (National Union of Mine Workers) to illustrate the difficulties that Muller faces in accurately describing the role of particular members. For instance (see p. 117) Muller does not mention that the sponsored group of miners' MPs have a rotation for speaking in major coal debates. So they take the speaking in turn. Thus Muller's statement about Ogden might have been correct for a few years when Muller reviewed the speaking activity of the members, but not for the full period that Ogden has served as a sponsored MP since 1964, and not for the present. Ogden is now chairman of the Miners' Group and since his election to Parliament in 1964 has spoken in turn and perhaps more often than others now in the group. One also should remember that the miners' union MPs are requested mainly to speak on coal issues.

In relation to sponsorship again, the Miners' Union has given us an unusual method for selection not mentioned by Muller. As the numbers of miners declined it was necessary for the sponsored members either to be dropped or sponsored in other ways. For a time the union shared the sponsorship of one of its MPs with the clerical union—(Clerical and Administrative Workers Union). Now the union-sponsored MPs are selected nationally.

In general the evaluation sections of each chapter should be expanded so that the author's meaning may be more fully appreciated. Just one illustration follows. The author's meaning in mentioning the Fabian elitism relative to the unions' nonsupport for Crossman (p. 97) needs clarification. There is an implication that the Fabian attachment is resented. But just what was Crossman's attachment? In addition, very early in the book, the author would assist the reader by giving a short explanation of how other affiliated groups direct their members of Parliament.

Although the general presentation of tables and information is very useful, there seems to be at least one section of the index which is missing from the beginning of the O's to Or.

Muller has produced a useful review of some parts of trade union activities relating to the Labour Party and governmental policy making and various unions' position on general issues. He has also provided a valuable section, even though with selected information, about communications. Yet how much and what type of communications are necessary for the unions to maintain with the MPs, when there may be only a few issues of concern in a parliamentary year? The volume should not be read as a final definitive work considering the space limitations of one volume. *The 'Kept Men'?* should be read in conjunction with other works and information about the unions, if one wishes to have a full view of the movement. While the final conclusions give a worthwhile review of the century and of relations between unions and the Labour party, and while it leaves us with some good questions about the next century, the conclusions should be read with care, and with full realization of the limitations of the material.

Muller has undertaken a large project and certainly offers a foundation for further evaluation and research. The next question now before the unions is first whether to, and then how to, sponsor representatives to the European parliament.

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System and Succession: The Social Bases of Political Elite Recruitment. By John D. Nagle. (Austin: University of Texas Press, 1977. Pp. x + 273. $17.50.)

John Nagle's work is a curious amalgam. It tries seriously to profit from both functionalist and Marxist approaches to elite recruitment problems. Nagel uses empirical data (not bad) and quantitative methodology (not badly) as his main tools to study class and generational factors in elite recruitment in four strategically chosen countries over the last half-century or more. He introduces a scheme for historical analysis that treats those countries for his purposes as equivalent political systems each of which, in its own milieu, functions at different stages under revolutionary conditions, benign and low-stress circumstances, or the highly stressful events of depression, purges, and unrest. He canvasses both pluralist and Marxist explanations for a variety of political succession problems. In general, he takes pluralists to task for down-playing the significance of elite composition; he asserts instead that occupational and generational compositional studies of political elites provide "fundamental indicators" of the nature of interest representation in a system. Rather succinctly he points up the unspoken ideological premises to be found in pluralist views on whether elites need to be "representative" or not; having done so, he alludes briefly and uncritically to the "underdeveloped" but self-evident Marxist view which makes the link between class composition of state elites and class representation of interests into an article of faith, citing the recent work of Bottomore and Miliband as welcome refinements. His own convictions both inform and bias his work: "It is simply not the case that all political elites are either equally unrepresentative or equally middle-class in either origin or occupation. The degree of representativeness may change over time within a system, and in either direction, and there is an absolute abyss between class origins of elite recruitment in capitalist and socialist systems" (p. 241).

Nagel's book is about leadership turnover patterns in four countries: Germany, Mexico, the United States and the Soviet Union. When examining the first two countries, he is tireless in pushing a Marxist formulation for what his findings mean; when discussing the latter two, class considerations dwindle in importance and he writes about generational displacement patterns instead. The American material is least well integrated. Thus, the "revolution in tenure" patterns in the American Congress since 1900 are examined in somewhat novel and suggestive ways, by using total career tenures rather than comparing mean tenure periods. But Marxist comments are scarcely needed. Thanks to the American two-party system for sponsoring political careers, the congressional recruitment process has persisted—even during such periods of reform capitalism as the New Deal or the Great Society years—in deriving its personnel so overwhelmingly from the upper social strata that it would be "a serious error to claim that elite background analysis can predict policy positions of individual actors" (p. 76).

Some of Nagel's most effective writing assesses various models for understanding Soviet elite recruitment patterns. Again, his Marxist commitment scarcely matters; he looks at Djilas' notion of "the new class" and at Trotsky's concern with "the bureaucratic stratum"; he inspects theories that emphasize rational-technical considerations in order to explain periods of rapid upward mobility, and a variety of kremlinological and formal models that stress patron-client mechanisms as critical factors. His own approach uses a generational model, which he plausibly argues has greater explanatory power than the others. No complications of the kind explored by Putnam, Prewitt-Eulau, or Suleiman in their recent works emphasizing generational mixes are introduced. But Nagel's discussion of the meaning of political generation is usefully substantive, cast in terms of people, events and issues that have special impact for unformed minds.

For his evidence about the social bases of political elite recruitment, Nagel has assembled bare-bones biographical information about age, occupational and educational background, and public officeholding records for American Congress members from 1790 through 1972, for German legislative deputies from 1871 through 1972, for the Soviet party elite from 1917 through 1971, and (thanks to Peter Smith) data about four elite layers—deputies, senators, governors and cabinet members—in Mexico from 1900 through 1970. As Nagel uses these four data sets to trace and compare "system and succession" patterns, he should provide the reader with more candid acknowledgement of their non-equivalence. These four longitudinal seminal cases, as he calls them, were chosen to "maximize the differences in culture and system-type, while assuring that elite recruitment is operating under conditions of relative autonomy" from foreign dominance. In each, two elite levels were to be tapped systematically—the "top political executive" and a somewhat larger "officially recognized body of effective national leaders" (p. 11). Only the Mexican data set closely fits this claim; in the other three data sets, members of relatively large constituted bodies (legislative chambers, party central committees, constituent assemblies) are virtually the only elite components represented. No political executive personnel, senatorial participants, or private-sector influentials are systematically sampled. In short, his data base is not as good as he tends to suggest.

More seriously, Nagel side-steps the complex but indispensable task of clarifying how the political and governmental machinery of four quite disparate and rapidly changing political communities actually worked. He attaches exaggerated significance to percentage changes of 4 or 5 points in the occupational composition of one legislature and its successor, as in his discussion of the German case where he juxtaposes occupational and birth decade information for the 1912 and 1920 Reichstags and inserts equivalent data for the 1919 constituent assembly that set up Weimar. He notes the doubling from 1912 to 1919 of worker-unionist occupations (15 to 30 percent) but it is characterized as suspect since all middle-class parties opportunistically put up unionist candidates as "window dressing" in 1919. A year later, worker-unionist ranks were down 4 points while businessmen-landowners were up 4. Nagel speaks of the "severe electoral losses" to the Left, "reduced recruitment of deputies from working class occupations" and the bolstering of "the social forces of the middle class." All this from a table that shows the former fluctuating from 15 to 30 to 26 and the latter varying from 19 to 14 to 18. When changes of 4 or 5 points are made much of, while sustained shifts of 12 to 15 points are made little of, the skeptical non-Marxist is likely to think darkly that the shortest distance between two debatable points appears to be a hard line.

Nagel does not discuss the recruitment process as a sequence of screening thresholds, apprenticeships, rivals, sponsors, campaigns, induction practices, institutional tutorials, or the like. Elite compositional changes tend to be seen as the only occasionally discernible, calculated effects engineered by a capitalist class. Of course, sophisticated Marxists do not hold that capitalists must be directly represented in government; they can be and often are more effectively served by managerial-executive and professional-technical personnel, through whom they effortlessly wield disguised power. Occasionally, too, the cynical right-wing parties of America, Mexico, or Germany have put up wageworkers as fronts. These two sets of considerations make it hard to use data, since much is done by proxy and many things are fake.

Nagel's book is occasionally provocative, sometimes persuasive, and too often merely hortatory. His rather flexible notions of methodology are often simplistic and even precious, but he shows a persistent respect for empirical evidence and a willingness to complicate his Marxist frame of reference which, in the event, does more than anything else to justify the conclusion that Nagel has written a book-length study with a reasonably coherent theme rather than what could easily have been a disparate set of article-chapters.

DWAINE MARVICK

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Comparing Urban Service Delivery Systems: Structure and Performance. Edited by Vincent Ostrom and Frances Pennell Bish. (Beverly Hills, Calif.: Sage Publications, 1977. Pp. 304. $17.50, cloth; $7.50, paper.)

Questions about the distribution of governmental power—which levels of government can most effectively provide which services?—are classical concerns of political theory. For local-level political systems, these issues have tended to center on the notion of tradeoffs between democracy and efficiency or between local control and equality of provision. Recent theoretical and empirical advances in public policy analysis have enriched such inquiry, especially regarding the linkages between government structure and performance. This volume of essays aims to add to our understanding of these linkages in the delivery of urban services.

The book consists of ten essays on service delivery systems in the local governments of post-industrial societies. These are preceded by a brief introduction in which Frances Bish does not attempt to establish any clear coherence among the set of essays. Rather, she merely highlights the key points made in each. Nine deal with the local governments in a particular country, and fully eight countries are treated. There is no attempt to develop generalizations across these various country studies. Only a few of the articles are rigorously comparative, in the sense of developing or testing theoretical propositions on multiple units of analysis, and only three employ empirixal data in explicit comparative analyses. Hence the "comparing" which occurs in most of these essays is extremely loose.

Two of the articles seem likely to interest a broad range of readers. In an illuminating essay, Vincent Ostrom opens the volume with a cogent explication of how public goods theory, and particularly the concepts of the public choice approach, can inform the analysis of governmental service delivery systems. Among other things, Ostrom distinguishes analytically between the set of individuals who control and consume the service (the "collective-consumption unit") and the production arrangements by which the service is provided. He argues that the preferred system of delivery for most services will be a quasi-market in which each citizen participates in an array of overlapping governments (as collective-consumption units) and selects among alternative production units for each specific service. Ostrom has articulated this perspective in other works, and aspects of his position were central to his recent exchange with Golembiewski in the December 1977 *Review* (71:1508–25). But this essay is a parsimonious and useful introduction to his conceptualization.

Roger W. Benjamin uses the English local government reorganization of 1974 as the backdrop for a suggestive theoretical exploration of the role of and the appropriate structure for local governments in a post-industrial society. He too applies the concepts and perspective of the public goods approach, and he generates a series of eminently testable hypotheses. The preference of post-industrial citizens regarding local services, Benjamin argues, will increasingly concern the issues of equity and participation rather than focusing primarily upon growth in the package of goods provided. Since he assumes that information failures increase with bureaucratic size, he posits that to the extent the delivery of a specific service is information-sensitive, the size-efficiency question should be resolved in the direction of smaller production units. Broadly, Benjamin contends that large, centralized political institutions, like those created in the English local government reorganization, are less well suited to the diverse wants of post-industrial citizens than the kind of policentric system of service delivery advocated by Ostrom.

Several other essays offer particular points of interest. Utilizing data from a sample of 36 Swedish communes, Bengt Birgersson identifies a "service paradox"—those communes which have higher levels on objective measures of service provision tend also to be characterized by lower levels of citizen satisfaction with the quality of service. This service paradox is intriguing, but plausible rival hypotheses which might account for this relationship are inadequately examined. Stromberg also uses the Swedish commune data to examine whether the elected local officials are representative, in attitudes and in attributes, of their constituents. Despite major differences in socioeconomic and personal characteristics, there is considerable correspondence between councilors and citizens regarding evaluations of the quality of services provided to the commune. In a tightly argued essay, Werner Pommerehne and Bruno Frey use public goods theory to generate certain hypotheses about the variations in production efficiency between public and private provision of local services. These hypotheses are assessed for empirical data on the refuse collection service in 103 Swiss cities. On the basis of carefully specified regression analyses, they conclude that public production has significantly higher average costs than private production of the service, that there are substantial scale economies for refuse collection, and that government action is necessary to insure continued competition among private producers of the service.

The remaining five articles in the book will be of interest primarily to area specialists. These are mainly institutional-structural discussions of the organization, reorganization, and service role of local governments in Israel by Daniel Elazar, in East Germany by Kenneth Hanf, in Italy by Philip Sabetti, in West Germany by Arthur Gunlicks, and in the Netherlands by Ruud Vader.

On balance, this volume contains rather less than one might expect from its title. The essays do not share a common framework, many of the essays are comparative only in the broadest sense, and few of the essays involve rigorous exploration of the linkages between structure and performance, at either the theoretical or the empirical level. If one wonders about the relationships between structure and performance in the delivery of local services or about the conditions which specify variations in those relationships, few answers are to be found in these essays. But one will find some interesting questions.

JAMES N. DANZIGER

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The Canadian State: Political Economy and Political Power. Edited by Leo Panitch. (Toronto: University of Toronto Press, 1977. Pp. xi + 475. $25.00, cloth; $7.50, paper.)

The Canadian State is a collection of articles on the historical and contemporary nature and activities of the state in Canada. Common themes found in the articles can be, in part, attributed to the fact that many of them originated in the faculty-student "Seminar on Contemporary Socialist Problems" held at Carleton University in Ottawa; some were presented in the political economy section of the 1976 annual meetings of the Canadian Political Science Association; and, prior to publication, most were read by other contributors to the volume. In short, the publication of the collection seems to have been preceded by a great deal of discussion among the contributors. As a consequence, there is a welcome thematic consistency often lacking in books of this nature.

In most of the articles, which range from a macro-analysis of the state, to the consequences of the division of state powers within confederation, to the composition of the state elites, there is clear evidence of the influence of analyses of the state undertaken by O'Connor, Miliband, and Poulantzas. More specifically, authors tend to deal with the "accumulation" and "legitimization" aspects of state activities at the federal and provincial levels. (Curiously, the "coercive" nature of the state is not dealt with to any great extent.) Equally important to virtually all authors is that the state should not be seen merely as " 'a committee for managing the common affairs of the whole bourgeoisie,' a caricature which fails to distinguish between the state acting on behalf of the bourgeoisie and its acting on their behest" (p. 4).

Despite a common orientation, the quality of the articles varies so much as to make an otherwise timely volume less effective than it might have been. Some inclusions, such as Stevenson's on federalism and the political economy of the Canadian state, Olsen's on the composition of state elites, and Pratt's on the state and development in Alberta, are clear and comprehensive, presenting either new materials or new syntheses of existing information. Unfortunately, some of the other inclusions are poorly written and convoluted, and really add nothing to conventional wisdom. In addition, there are a number of articles in which a presumed Marxist approach adds little to the analysis; the discussions could easily have been formulated by unimaginative non-Marxist scholars. Indeed, many aspects of the argument have been presented more forcefully by "bourgeois" scholars. It takes more than a few theoretical quotations in an introduction and conclusion to make a sound piece of Marxist scholarship. Stripped of introductory and concluding comments, in fact, some of the articles emerge as poorly executed traditional pressure group analyses.

There is also a conceptual naivete that reduces the credibility of some of the articles. A number go to great lengths to point out that a monolithic view of the state is erroneous. There is often conflict among different segments of the bourgeoisie and this conflict finds manifestation in the state. But who ever thought it was otherwise? Certainly not traditional pluralists who viewed the state as a broker of interests. Certainly not astute Marxist analysts. To whom, then, is this view attributed? In making so much of an image of the state that perhaps few of us, Marxist or otherwise, hold, a number of contributors set up a straw man that does little to convince the reader of the validity of the remainder of the argument.

Despite the volume's obvious weaknesses, a good number of the articles in *The Canadian State* clearly add a great deal to a Marxist appreciation of Canadian society. They should therefore be read by all serious students of Canadian politics and society. Equally important are the number of questions that arise from a reading of some of the articles; they will no doubt inspire further inquiries into the political economy of Canadian development.

J. PAUL GRAYSON

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The Politics of Bureaucracy: A Comparative Perspective. By B. Guy Peters. (New York and London: Longman, 1978. Pp. ix + 246. $12.50, cloth; $6.95, paper.)

Comparative politics is a well-established and reasonably well-ordered branch of political science. The same cannot be said for comparative administration. Ferrel Heady who surveyed this putative subfield recently concluded that after a period of florescence in the 1960s and early 1970s, matters are now pretty much in disarray ("Comparative Administration: A Sojourner's Outlook," *Public Administration Review* 38: 358–65). Guy Peters, however, has managed to tackle successfully one particular aspect of comparative studies of administration and bureaucracy: patterns of bureaucratic politics. He has provided students of comparative administration with a most useful and well-written volume in which the existing literature is neatly codified and just about all important topics receive attention.

In confining himself to a little less than 250 pages, Peters had to adopt a number of self-denying ordinances. The bureaucratic politics depicted in this book is chiefly of the "Western" variety, references to non-Western experiences notwithstanding. The citations at the end of each chapter are chiefly to the literature in English. The book's attention is focused on the process of bureaucratic politics and thus cannot discriminate in detail among the consequences of different substantive policy issues. Yet the book can render excellent service to those who desire an introduction to a central political issue of all modern states: the place of public bureaucracies in policy making and the general political process.

The central issues of the politics of bureaucracy are treated in chapters 6, 7, and 8. In these chapters Peters examines the relationships between bureaucrats and other political actors/organizations and addresses in more generalized fashion the problem of administrative accountability. In the first five chapters Peters lays the foundation for these central issues. Following an introductory chapter in which basic terms are defined, successive chapters are devoted to an historical overview (chapter 2); the importance of the linkage between political culture and administration—a concern not often addressed in administrative studies (chapter 3); recruitment (chapter 4) which confronts the issue of a representative bureaucracy in a sane and balanced manner; and administrative structure (chapter 5) which examines such organizing principles as area, process, and clientele.

Principal attention, however, is given to the treatment of the politics of bureaucracy in the book's three concluding chapters. The political actors which interact with the bureaucracy are classified as "informal" and "formal." Chapter 6, devoted to the former, is limited to relations between bureaucrats and pressure groups, with only a passing reference to political parties and some of the problems of one-party systems. The kinds of interactions examined in this chapter fall into four types: legitimate; *clientela; parentela;* and illegitimate. The second and third type are, of course, adapted from the standard work of LaPalombara. The more nearly novel aspect of this analysis of bureaucrats/pressure group relationships is the first type which Peters labels "legitimate" and denotes with this label those arrangements in which pressure groups are "legally and officially in the process of making and administering public policy" (p. 143). West Germany, along with the Low Countries and Scandinavia, are cited as principal examples of countries where such relationships exist. The use of the term *legitimate*, however, is clearly idiosyncratic, for in other systems, such as Italy, *parentela* and *clientela* relations are perfectly legitimate. It would seem that all kinds of relationships can be considered legitimate in a society and not just those so labeled "legally and officially." It always poses problems when one uses in a technically restrictive manner terms that have a more general meaning. Finally, a word about illegitimate bureaucratic/pressure group relationships. The author wishes to designate with this term interactions that are "outside the pale of normal political action" (p. 155). One might have expected that the reference would be solely to acts of violence or terror as efforts to influence the bureaucracy, but that is not the case. Rather it means that in a given system some pressure groups might not be acceptable while others are; it is also used to designate a non-pluralist system in which no pressure groups are legitimate. Thus an illegitimate pattern of bureaucrat-pressure group interaction could be anything from a situation where a given pressure group is free to exist but receives no hearing and support from the bureaucracy to one in which a pressure group uses violence and terror. The category simply tries to cover too much ground and thus lacks the specificity that characterizes some of the other three types.

Chapter 7 which examines the power relationships between the bureaucracy and formal political institutions contains the heart of Peters' analysis of bureaucratic politics. He begins by laying out the resources at the command of the two sets of participants in the power game: legitimacy, the power of the purse, and the grant of latitude or discretion on the part of the political institutions; information and expertise, decision power, availability of political supporters on the part of the bureaucracy. By using these resources, the two contending groups can affect the outcome of the contest. But though the contest might seem to be weighed in favor of the bureaucracy, specific outcomes can be, and are, influenced by the degree of integration of the bureaucracy; the presence/absence of rivalry between parts of the bureaucracy; the degree of politicization of a given issue; and the nature of the agency or agencies' output. Both bureaucracy and political institutions use a number of "ploys" or strategies to affect the outcome of the contest. The bureaucracy's role in planning—an increasingly important process in modern political systems—its ability to manipulate the technical side of the budgeting and programming process and its role in advisory bodies, all can be brought into play to score successes for the bureaucracy. The political executive, on the other hand, tries to counter bureaucratic strategies through special budgetary institutions like OMB, through staff controls or even through the use of counter staffs, as is increasingly the case for the U.S. Congress.

All these strands of bureaucratic politics are brought together in chapter 8 which is devoted chiefly to an investigation of how the political arm of government attempts to control the exercise of power by the bureaucracy. The chapter travels over well-established ground in examining external controls of an organizational kind, political and public pressure. The usual categories of legislative and executive control are enumerated; and finally the issue raised in the Finer-Friedrich debate over the efficacy of internal (normative) versus external controls is reexamined. That dispute has remained unresolved—as it should remain—and the present study is not able to push much beyond these points.

It is to Peters' credit that he has avoided a "technocratic gloom and doom" stance. He has the pluralist's healthy optimism that it is possible to operate a society and a policy without having to suffer the excesses of faction and the consequences of unbridled power drives by either bureaucrats and politicians. Who among the predominantly pluralist political science profession in the United States would be entitled to cast the first stone?

Peters has performed a useful task with skill; he has marshaled a variegated literature in a systematic manner; and he has put down another stone for the "yellow brick road" that we need to travel towards a more comprehensive and more theoretically informed comparative study of administration and bureaucracy.

ALFRED DIAMANT

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Cambodge annee zero: document. By François Ponchaud. (Paris: Juillard, 1977. Pp. 250. FF 42.)

This is a stirring book about the Khmer Communist revolution by a French priest who served both in the Cambodian countryside and Phnom Penh from 1965 until the Communist seizure of power in 1975. On April 17, 1975, Father Ponchaud was at his final post in the student foyer he directed in Phnom Penh when the Khmer Rouge entered the city, and for another day he remained outside the French Embassy compound in which Westerners took refuge, permitting him to observe, and even drive around the city as a guide, some of the young revolutionaries who were billeted in his foyer. He remained at the French Embassy until May 6th, when he was evacuated from the country with the other foreigners who were lodged there. A few months later, in France, Ponchaud set about the task of gathering materials for this book, drawing upon his long experience and his intimate knowledge of the language, history and culture of Cambodia. He interviewed scores of Khmer refugees in France and Thailand, and collected written testimony from 77 refugees in Thai camps and 17 refugees in Vietnam. He also monitored the daily radio broadcasts from Radio Phnom Penh, the official voice of the new Kampuchea.

The first section of the book is written in the form of a personal memoir. In the subsequent sections Ponchaud quotes refugees at length and adds his own eyewitness testimony, giving a moving account of the Khmer revolution—more sweeping in its rapidity and scope than the Russian, Chinese or Vietnamese revolutions. This violent change has included an emptying of the cities, large-scale liquidation of designated enemies, a complete restructuring of the society and the economy and an attempt to abolish religion and to remake the culture. When it first appeared, the book aroused controversy over Ponchaud's estimate that the number of deaths during "peacetime" exceeded one million (in a country whose population was estimated at 7½ to 8 million). The rigid isolation of the new regime has made it impossible for independent observers to arrive at a precise figure for the number of deaths that might be attributable to the policies of the new regime, but consistent refugee reports since the book was published leave no doubt about the horrendous toll of lives that has resulted from forced marches, disease, malnutrition, exhaustion and execution of "enemies."

Ponchaud offers valuable cultural insights that help deal with the puzzle of how the revolutionary leaders could transform a country known for its *donceur de vivre* into a vast *gulag.* He challenges earlier Western clichés that have characterized the Khmer as only "sweet and peaceful" and describes an aggressive dimension of their character which has manifested itself in geographical expansion, particularly during the Angkor period, and in their reputation in Southeast Asia as fierce soldiers. The cruelties inflicted by the Lon Nol regime upon its enemies, and the massacre in 1970 of thousands of the Vietnamese minority residing in Cambodia, indicate that excesses were not confined exclusively to the Communist side. While Theravada Buddhism preaches gentleness, serenity and pity for the less fortunate, Ponchaud notes that in Cambodia the notion of pardon can be interpreted as a sign of weakness and cause a "loss of face." An enemy can be regarded as thoroughly evil and if one recognizes virtue in him, one becomes his accomplice. When these tendencies were united with a zealous revolutionary Marxist ideology, they led to the worst excesses, with the victors unable to pardon the vanquished who had made them suffer.

Ponchaud suggests other cultural factors which help to explain how three to four million city dwellers meekly obeyed Khmer Rouge soldiers' orders to leave their homes, and how a small number of cadres could command thousands of refugees. The Hindu tradition within Cambodia, still an important influence, considers authority to be a divine incarnation which must be respected. (The earlier regimes of Prince Sihanouk, as well as Lon Nol and his collaborators, profited from this respect and abused their authority, notes Ponchaud, revealing his own distaste for the predecessor regimes.) In overcoming their own individual reticence to launch their revolt, the revolutionaries drew inspiration from a radical Marxist ideology, and once in power they took advantage of the instinctive tendency of each Khmer to obey.

The author gives valuable information about the origins and French education of some of the key revolutionary leaders. While his account is still sketchy, it is particularly useful since these leaders have deliberately shrouded themselves in mystery, ruling for several years through an anonymous *Angka* (revolutionary organization) which they did not even acknowledge as being led by the Communist party of Kampuchea until the fall of 1977. He also gives insights into the doctrinal zealotry and methods of the new leaders, who have abolished private property, dispensed with money as a medium of exchange, eliminated mass circulation newspapers, limited classroom instruction in schools to a few hours daily so that "practical" education in the fields could be conducted, and most importantly, have made an implacable commitment to national self-sufficiency which is characterized by their slogan, "If we have rice, we have everything . . . everything should serve rice production."

The French edition is written with verve, presenting a breathtaking, deeply disturbing account of what must be the most radical revolution of the twentieth century. (By the time this review appears, an American edition, published by Holt, Rinehart and Winston will be available. An English hard-cover edition has been published by Allen Lane, 231 pp., £6.50, and a paperback edition by Penguin.)

JOSEPH J. ZASLOFF

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Christian Democracy in Western Germany. By Geoffrey Pridham. (New York: St. Martin's Press, 1978. Pp. 371. $19.95.)

In view of the scarcity of works on the parties of the European Right and Center, Geoffrey Pridham's work is welcome. Part 1 of the book provides a historical analysis of the political and organizational development of Christian Democracy, focusing on the impact of the Government/Opposition roles on the party from 1945 to 1976. Part 2 adds several chapters on the composition and structure of the CDU/CSU, covering mostly familiar ground. Chapter 10, a profile of the Bavarian CSU, is especially useful for American readers.

Pridham distinguishes among three distinct phases in the development of the CDU/CSU: (1) the 1950s, when the party accumulated impressive electoral and political achievements under Adenauer's firm guidance; (b) the 1960s, marking a period of transition and decline, revealing a lack of authoritative leadership and an inability to adjust to a new political environment; (c) the 1970s, when the CDU/CSU found itself in the opposition, a role which it was ill prepared to perform. Throughout most of this development it remained a prototype of the modern catch-all party, or *Volkspartei,* with its dependence on charismatic leadership, its low level of organizational and ideological integration, a strong electoral orientation, and an emphasis on foreign policy.

There can be little doubt that the shape of Christian Democracy in Western Germany would have been different had it not been for the towering figure of Konrad Adenauer. While Pridham devotes considerable space to an exploration of Adenauer's contribution, he fails, I think, to clarify the source of the chancellor's authority (other than referring vaguely to his charismatic authority). The implications of Adenauer's leadership for the future development of the party emerge quite clearly in Pridham's account. For example, Adenauer's pragmatic style became a legacy in that it thwarted efforts at defining an ideological foundation for the party after World War II. A case in point is the commitment to Christian values. Beyond the obvious electoral appeal of the capital "C" in the party's name, it remained unclear just what the distinguishing features of Christian politics really were. Perhaps Pridham should have examined this and other ideological tenets more closely. The lack of an ideological core reduced the party to an essentially reactive role in the Grand Coalition.

In addition, Adenauer's lack of interest in organizational matters thwarted the party's development. Of course there were other reasons as well, such as the reality of federalism and a widespread ideological antipathy to a *Parteiapparat.* Pridham contributes some interesting evidence showing that this indifference was not always shared by party activists at regional and local levels (p. 248 ff). But the national leadership was content with a party organization that was in fact little more than a "Kanzlerwahlverein." Interest in party reform and development flared up whenever the CDU/CSU's electoral fortunes declined. Only the opposition years forced the party to engage in the long overdue effort of ideological and organizational development.

From the title of the book, the reader expects at least some exploration of external issues, notably, how did the CDU/CSU in its governing role shape the character of the West German political system? The political environment in which the party operated was not simply given, but shaped in important ways by the party's control of the government in the first 20 years of the Federal Republic. A second shortcoming relates to the absence of any theoretical speculation. As a case study, *Christian Democracy in Western Germany* contains a good deal of evidence to suggest comparisons with other catch-all parties as well as empirical generalizations about the development of political parties and party systems. Unfortunately, Pridham attempts neither.

Short reviews often confuse rather than clarify the merits of a work. Pridham's book contributes considerably to our understanding of the CDU/CSU since Worls War II. But it is unfortunate that he has defined his theoretical task so narrowly.

JUTTA A. HELM

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The Expansive Elite: District Politics and State Policy-Making in India. By Donald B. Rosenthal. (Berkeley: University of California Press, 1977. Pp. xiii + 348. $16.75.)

India's failure to achieve the kind of income distribution that its national political leadership advocates is a problem that has seized the attention of both American and Indian scholars, along with a host of international donor agencies committed to greater equity in the development process. Not that India's income distribution is worse than that of other developing countries; to the contrary, India's bottom 40 percent earns 20 percent of the national income, according to the much-quoted study by Irma Adelman and Cynthia Taft Morris, *Economic Growth and Social Equity in Developing Countries* (Stanford, 1973), well above Sri Lanka, Mexico, Brazil, and even better than Israel and Japan. The top 5 percent earns 20 percent of the income, again less than that of a majority of the developing countries. India, according to the joint study by the World Bank and the Institute of Development Studies, *Redistribution with Growth* (Oxford, 1976), is on a par with (or is more equalitarian than) Tanzania, Lebanon, Puerto Rico, Holland, Norway, the Federal Republic of Germany, and Sweden. But India's per capita income remains so appallingly low that those who live in the bottom 40 percent are beneath any acceptable standard of dignity and decency, making the disparities between the rich and poor even more unacceptable than elsewhere. Nonetheless, it is well to note that income disparities in India are not as severe as in other developing countries, and that the problem of poverty is more a question of *absolute* living standards than that of *relative* shares. In short, while the redistribution of income through a leveling process would obviously result in greater equality, its impact on the income of the poorest would be nominal.

Donald Rosenthal's book, *The Expansive Elite,* is not concerned with the larger question of why India's overall growth rate has been low, but rather with the question of why the *benefits* of rural development are so inequitably shared. His central thesis is that popular participation in the political process did not result in more social and economic equality, but instead increased the power of local rural elites who successfully took control of the new political institutions. He argues that "the benefits of rural development in such vital areas as agricultural investment, educational opportunity, and rural local government went principally to a relatively small segment of the rural population" (p. 4), a group that he calls "the expansive elite."

To argue this case, he looks selectively at the government programs in Kolhapur and Poona districts in the state of Maharashtra. He begins with a political overview of politics and policy in the state as a whole, then turns to a brief description of the political economy, political leadership and political conflict in the two districts. A central theme is that in both districts new elites took control of the local Congress party organization, the District Land Development Bank, and a variety of other agricultural credit societies and cooperative organizations. The theme is further developed in a chapter on the Congress party in which he describes who has "made it" politically, how, and with what purpose. There then follow three case studies of specific policy arenas: education, cooperatives, and local government. In a chapter on educational policy he shows that the new, more affluent rural elites successfully won the battle for achieving access to higher education, with little regard on the one hand with "quality" in education, or on the other hand, with achieving universal primary education that would benefit the lowest income groups. In two chapters on the cooperative movement, Rosenthal argues that the larger market-oriented producers received most of the benefits from the cooperatives, to the neglect of those marginal farmers and landless laborers who were not considered "credit worthy." Cooperatives were in no sense socialist. On balance, says Rosenthal, they probably contributed to inequality within the countryside by widening income disparities, since only the market-oriented farmers took advantage of the credit made available by these institutions to link themselves into a growing system of commercial agriculture. Finally, in a chapter on local government institutions, Rosenthal argues that the state government has been reluctant to expand the powers (and resources) of popularly elected local bodies, but that in any event these institutions are largely controlled by the higher-status rural classes.

In a concluding chapter Rosenthal argues that policies in each of these areas, and the new institutions created by these policies, simply provided opportunities for individual politicians and to maximize their own personal ambitions, and to benefit those most closely associated within them, without bringing about any major social transformations along distributive lines. "Rural local elites . . . have become a source of institutional restraint, standing in the way of more participatory and more redistributive patterns of political development" (p. 323).

By now, Rosenthal's argument is not new, though by focusing on how rural elites and institutions function at the district level he provides a degree of detail and analysis not provided in other studies. But there are several features of the argument that remain unsatisfactory. The first is the question of how much the rural elites have in fact actually benefited, not just in comparison with the landless laborers and the marginal farmers, but in comparison with the organized urban sector, the middle classes, and the bureaucracy itself. Second is the question of whether the market-oriented rural sector is itself expanding, that is, whether the size of the "expansive elite" is growing or contracting in size? And third is the question of whether an improvement in the well-being of the lowest income groups in the countryside can only take place, as Rosenthal argues, by "destroying the rural social structure" (p. 326), as if its destruction would ensure an improvement in the absolute living standards of the poor. Since Rosenthal is dubious over the prospects that the lower classes will be politicized to demand a greater share in the benefits of development, he believes that what is required is a "more effective set of organizational weapons at the command of a national leadership responsive to societal needs and capable of converting programmatic goals into implementable policies" (p. 328), presumably by creating some kind of radical cadre party. Others might define the policy problem in terms of how one might create new opportunities in education and employment for the lower strata without destroying the opportunities for the market-oriented rural classes? Why must the policy problem be defined in zero-sum terms?

MYRON WEINER

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The Moral Economy of the Peasant: Rebellion and Subsistence in Southeast Asia. By James C. Scott. (New Haven: Yale University Press, 1976. Pp. ix + 246. $15.00.)

Increasing academic interest in peasants and their rebellions has given rise to an outpouring of theories intended to help explain rural insurrection. The explanations can, very roughly, be divided into two categories. One powerful line of analysis—represented by the work of Barrington Moore, Eric Wolf, and Jeffery Paige, for example—is a structural one in which rural uprisings are analyzed primarily as a function of class coalitions and conflicts. A second approach—and here Chalmers Johnson's *Peasant Nationalism* and the writings of Franz Fanon come to mind—has focused less on objective social structures than on the question of peasant motivation, the subjective processes believed responsible for peasant revolt. James Scott's book is an important addition to this second line of analysis.

To be sure, Scott himself pays homage to the structuralists, crediting their approach with a notably successful explanation of the causes of peasant revolution. By contrast, his own "moral economy of peasants" is explicitly designed less to explain when peasants rebel than to tell us "what makes them angry and what is likely, other things being equal, to generate an explosive situation" (p. 4). In exploring this "social dynamite" of rural life, Scott favors a phenomenological perspective. His intent is to penetrate the fabric of peasant moral sentiment, to lay bare the criteria of justice and injustice as envisioned by the peasants themselves.

Starting from a picture of peasant life as chronically impoverished and dangerously insecure, Scott posits the "subsistence ethic" as the cornerstone of peasant morality. The subsistence ethic, he reminds us throughout the book, is a rational principle which assigns priority to the assurance of a minimum livelihood. Peasants, according to Scott, strive not to maximize profits, but to reduce the risk of falling below a bare survival income. They thus judge the fairness of taxes and rents, for example, by the degree to which such extractions permit the maintenance of a subsistence level. Arrangements which provide special dispensations or subsidies to tide the peasant household over a bad harvest will be viewed as less exploitative than arrangements which lack such security measures, even if the latter would on the average extract a far lower payment from the peasant. According to Scott, many of the institutions of precapitalist village life were based upon this conception of peasant justice. Redistributive social arrangements among villagers and reciprocal patron-client obligations between landlords and tenants are identified as examples of traditional mechanisms designed to ensure a subsistence to all peasant families. However, two monumental historical developments—the expansion of Western capitalism and the rise of the modern state—are seen as having worked systematically to undermine these preexisting methods of social insurance, thereby violating peasant norms of justice and opening the door to principled resistance. Case studies of the Saya San Rebellion in Burma and the Nghe-Tinh Soviets in Central Vietnam, movements which developed in response to the Great Depression of the 1930s, are introduced to demonstrate the insurrectionary potential of peasants whose traditional norms had been undercut by a combination of state penetration and the advance of the world market.

While the case studies do not exhibit the richness of research that one might have hoped for, they do illustrate the utility of analyzing rural rebellion within the context of large-scale historical change. Less obvious, however, is their utility for establishing the book's "moral economy" thesis. While we may all agree that the Depression ushered in a period of acute hardship for peasants in newly commercialized areas, are we justified in interpreting the eruption of rural protest as a reflection of peasant indignation against an unprecedented erosion of subsistence guarantees by landlords and the state? If so, how do we explain the multitudes of precapitalist peasant rebellions waged across the globe for centuries prior to the advent of modern market or state? Countless such rebellions cast doubt on any assumption that a subsistence ethic was either shared or fulfilled by traditional governments or upper classes. Moreover, examples of precapitalist peasant uprisings which advocated radical land reform suggest that peasants could sometimes act in support of class interests that went well beyond a simple plea for subsistence tenancy. A picture of traditional peasant society as a world in which notions of reciprocity and subsistence reigned runs the risk of underestimating the contradictions inherent in the landlord-tenant relationship. How effectively redistributive mechanisms or patron-client ties operated to ameliorate such tensions is far from certain. In the case of North China, for example, some recent research has suggested that village solidarity there *originated* as a response to the deterioration of peasant life attendant upon the intrusion of capitalism. Redistributive social arrangements thus may not have constituted an enduring institution founded in a feudal conception of reciprocal obligation. In any event, Scott does not present us with the necessary evidence to settle the case convincingly.

The strength of the book lies in its explanation for some of the seemingly "irrational" aspects of peasant society, such as the tendency to labor intensively for extremely low marginal returns, the reluctance to adopt new agricultural technology, and the willingness of the land-poor peasant to pay unduly high prices for additional land. Picking up a theme of Clifford Geertz and others, Scott shows that these patterns are not the product of any peculiar peasant mentality, but are eminently rational choices for persons living near the margins of survival with few alternative productive outlets. In this respect Scott's work is a welcome antitode to certain anthropological writings which picture a unique "peasant personality." Like these other works, however, this book may also be faulted with a tendency to homogenize unduly the peasant situation. How, for example, do we explain the instances where peasants *have* eagerly adopted cash cropping? And how are we to account for those peasant rebellions that *have* gone beyond claims for the restoration of subsistence rights to press for more radical demands? While Scott points out that his arguments apply with greatest force only to peasants "with very low incomes, little land, large families, highly variable yields, and few outside opportunities" (p. 25), his propensity throughout the book to refer to "the peasant" tends to obscure the many significant differences within this category. Indeed, one of the major contributions of recent research on rural society has been to highlight the central importance of structural and ecological variation. Tenants, freeholders, part owner/part tenants, poor peasants, middle peasants, and rich peasants hold quite distinct interests that influence both their agricultural strategies and their involvement in rebellion. Likewise, the physical setting—climate, soil fertility, proximity to transport facilities, accessibility to government control—have decisive implications for the mode of production as well as the mode of collective violence. While Scott does make reference to thsee factors (p. 97, for example), the fact that they are not assigned the status of independent explanatory variables, used to account for the rich variation that we know to characterize peasant society, detracts substantially from the power and applicability of the analysis.

For the political scientist interested not merely in the question of "why peasants become frustrated," but also in the question of "why peasants rebel," Scott's book is a disappointment. Although he explicitly disassociates his approach from "frustration-aggression" or "relative deprivation" explanations of rebellion (p. 187), Scott shares with these other approaches a tendency to underplay the political context. Thus the book is largely silent on the organizational and mobilizational aspects of peasant rebellion. Repeated reference is made to R. H. Tawney's metaphor of the (Chinese) peasant as "a man standing permanently up to the neck in water, so that even a ripple is sufficient to drown him." This image of a helpless victim, while a poignant reminder of the overwhelming forces to which the peasantry was subjected, does not convey a sense of the cooperative resiliency represented in the efforts to rebel against this fate. Although we are told that anger, while necessary, was insufficient to buoy the drowning peasantry up in a revolt, we are given little sense of the political dynamics involved in the process.

Scott offers much insight on the motivational makeup of certain kinds of peasants under certain historical circumstances. To determine just who, when, and so what, we are drawn back once again to the structuralist point of view.

ELIZABETH J. PERRY

University of Washington

Peasant Politics: Struggle in a Dominican Village. By Kenneth Evan Sharpe. (Baltimore: The Johns Hopkins University Press, 1977. Pp. xix + 263. $15.00.)

Kenneth Sharpe provides us with a thoughtful study of political culture and political action in a "peasant" community in the Dominican Republic. The book's central question asks how a group of coffee growers come to recognize the symptoms of their social "malaise" as a significant economic and political problem in the allocation of power, and how—once *conscientizados*—they come to act upon it through community organization. Empirically, Sharpe narrates the successful (for some) creation of a coffee-growers' cooperative, designed to curtail the monopoly power of local middlemen. Theoretically, he tries to weave through the narrative a view of the "political economy" of Dominican coffee production and exchange and the "phenomenology" of peasant life, demonstrating along the way the importance of culture and ideology in the overall process of social change. Stylistically, he brings a literary flair to conventional social science usage, making the ethnography of *Peasant Politics* very readable even while explicit theory-building continues to suffer the abuses of sociological codes ("strain" theory versus "interest theory").

The book is divided in two parts. Part 1 restructures the ethnography of peasant life in Jaida Arriba, Dominican Republic. Chapter 1 dramatizes vividly a day in the life of a peasant, introducing *dramatis personae* who reappear as actors throughout the book. Chapters 2 and 3 describe the telescoping of control in the international chain of market exchanges and quotas which ultimately determines coffee prices at the local level and establishes the parameters in which debt and credit relationships are played out between peasants and middlemen. Sharpe relates middleman control (over credit and, hence, access to consumer goods) to the pressures on different classes of coffee growers derived from forms of production, demography, and inheritance patterns. He introduces new dramatis personae to portray—albeit somewhat loosely—the various responses of agricultural day laborers, small-holders, middle-holders, and large-holders. Chapter 4 outlines the careers of middlemen in the community and traces the first elements of competition among the middlemen which eventually provided the opening for successful organization of producer and consumer cooperatives. Chapter 5 narrates the organization of these cooperatives, emphasizing the role of church leaders in breaking the deadlock which "traditional" religious beliefs and dominant patronic relationships placed upon the peasants. It gives credence to the liberating power of information in forming horizontal community alliances and countering the legitimating effects of former asymmetrical patron-client dyads.

In part 2 of the book, Sharpe tries an approach to theory, albeit one tied closely to the narrative. Chapter 6 begins with an intelligent discussion of ideology, framing a hypothesis which orients the rest of the book. Leaning heavily on Clifford Geertz's definition of culture (and less so on Paulo Freire's notions of *conscientização*), Sharpe searches for the "meanings" different classes of peasants give to their social situations and explores how these are generated, sustained and ultimately changed. Ideologies, he argues, are "symbolic guides" which orient peasants' behaviors critically and forcefully when their perceptions of their situation turn from the acceptance of "malaise" to an objective analysis of solvable problems. Thus, in chapter 7, Sharpe examines how these Dominican coffee growers reacted to their depressed economic condition prior to 1967, and what induced them thereafter to struggle for change. He discusses the "moral economy" which previously provided legitimacy to asymmetrical relations between peasants and middlemen, identifying a series of "obstacles to problem definition and ideology formation." Chapter 8 probes the differing acceptance and rejection of the cooperatives among individuals at different class levels, touching on the relationship between clerics, government organizers and local leaders as they introduce new ideas of adequate "coping" strategies to the community at large. Chapter 9 tells how traditional market controls were broken and how the power to engage effectively with national cooperative leaders was engendered locally in the process. Chapter 10 provides a useful summary of the major lines of argument in the book and a brief discussion of methodology.

In all, this is a useful case study which outlines the structural problems inherent in local community organization and development and helps us to understand the complexity and pitfalls of organizing at the "grass roots." Despite its concern with the integration of micro and macro approaches, it falls short of a thoroughgoing political and economic analysis, confining itself to the analysis of a single set of institutions at the local level. "Political economy" requires more than a "top-down" view of marketing relationships, and Sharpe fails to relate coffee production and distribution to the broader Dominican economic system. Likewise, he does not treat the larger issues of Dominican society and polity, although he hints at their importance in the conclusions. By sustaining this essential "view from the bottom," Sharpe entraps himself in an advocacy of self-help and voluntarism, made effective by a heavy dose of tutelage.

SHEPARD FORMAN

Rio de Janeiro

Praxis: Marxist Criticism and Dissent in Socialist Yugoslavia. By Gerson S. Sher. (Bloomington and London: Indiana University Press, 1977. Pp. xix + 360. $15.00.)

"Philosophy is introduced into the world by the yelling of its enemies who betray their internal infection by their noisy call for help against the blaze of ideas." Marx's nineteenth-century observation on the politics of culture is today relevant for Eastern Europe, where the periodic display of critical and humanistic forms of Marxist thought has been followed by more or less routinized acts of political repression against such critics as Kolakowski, Baumann, Kosik, and Heller. For a time, it seemed, Marxist criticism and dissent enjoyed reasonable prospects for survival as a social institution only in Yugoslavia. A receptiveness in that country to new ideas and institutional forms (excluding much popularized works by Milovan Djilas and Mijahlo Mihajlov which lie outside the Marxisn tradition) appeared for a time to ensure that Yugoslavia would persist as a kind of cultural protectorate for contemporary Marxist humanism. All of this changed in early 1975 with the closing of the journal *Praxis* and the externally dictated removal of eight of its contributors from Belgrade University, events which seem to mark the close of an era in which self-criticism and public dissent were a way of life in at least one socialist society.

Gerson Sher's *Praxis: Marxist Criticism and Dissent in Socialist Yugoslavia* is a book about the origins, development, and repression of the ideas and activities of a group of Yugoslav philosophers and sociologists whose work and public life revolved around the journal *Praxis* in the years 1964–1975. Sher's aim is to document and interpret the philosophical, political, and cultural significance of *Praxis* and related social institutions (e.g., the Korcula Summer School) in this period. Sher also seeks to create a broader and more informed appreciation of political and cultural trends in Yugoslavia and to provide a basis for the revaluation of Marxian theory from a democratic and humanistic perspective (p. ix).

Sher's *Praxis* succeeds in achieving these aims better than any existing work now available in English (or Serbo-Croation). The clarity, force, and informed sensitivity which he brings to his subject deserve serious attention and respect. Sher, the translator of Svetozar Stojanović's *Between Ideals and Reality*, has a firm grasp of primary sources and the language competence to interpret them in a way that preserves their intended meaning. Equally noteworthy is the important gap that this book fills: it is the first thorough and detailed account of the structure and content of Marxist dissent in Yugoslavia. Sher's method itself illustrates the fine results that may be produced by consistently applying a humanistic, critical, and dialectical perspective to a complex and difficult subject. Accordingly, Sher's *Praxis* avoids the interpretive excess that often stems from a one-sided personal commitment to particular values. Sher's interpretations—which are self-reflective and critical in the best sense of those words—are likely to resonate very well with those of Yugoslavs as different in outlook as contributors to *Parxis* and members of the Central Committee.

Sher's central argument is that *Praxis* played a significant if socially limited role: the journal constituted a focal point around which diverse contributors developed a sense of identity; it provided a medium for the critique and transformation of the contemporary Marxian world view; it helped to diffuse new ideas among a relatively small but influential readership that included top party and government leaders; and it recurrently captured public attention in times of crisis. As a vehicle for sustained Marxist criticism and cultural change, *Praxis* was only partially successful: its essentially academic content, combined with intermittent government repression, produced a small and select readership; its role in generating and legitimizing intraparty dialogue and self-criticism was scarcely felt in society as a whole; the journal's aim of cultivating a critical stance toward socialist theory and practice—which was accompanied by explicit disclaimers of any interest in everyday politics—did not succeed in persuading opponents (including "liberal" arch-enemies like Mike Tripalo) that the journal was not a symbol or even organized center of political opposition. The founders of *Praxis* had chosen a politically ambiguous structure of dissent, the "thick journal," which was conceived as a medium of cultural criticism in a society whose revolutionary movement was frequently arrested by specifically Marxist forms of cultural lag. In one sense the journal was "apolitical"; in another it represented "a political act of the first order" (p. xviii).

If Sher's *Praxis* contains any inadequacy worthy of note, it is in the area of historical prognosis. Thus, it is not entirely clear what evidence or assumptions permit the conclusion that the period after Tito is likely to be one in which "spontaneous, honest social debate and critical thinking—elements which were manageable during Tito's lifetime by virtue of his own immense authority and prestige—will become memories of a past age: the age of Praxis" (p. 241). On the contrary, it seems plausible that the age of *Praxis* only began in the years 1964–1975, a period when the vitality as well as contradictions of Yugoslavia's unique socialist revolution began to be fully evident. A closer reading of postwar Yugoslav history might well yield the conclusion that the sociocultural context that fostered "self-managing socialism" also nurtured the growth of *Praxis* itself. A society that has experienced Marxist criticism and dissent as a social institution has not depended on *Praxis* or any other journal for its creative thrust: it is perhaps the other way around. Indeed, the creative and self-transforming elements of Yugoslav socialism have not even depended on Tito and top political figures, however, much their reactions to the "blaze of ideas" might suggest the end of an era.

WILLIAM N. DUNN

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Burma: Military Rule and the Politics of Stagnation. By Josef Silverstein. (Ithaca, N.Y.: Cornell University Press, 1977. Pp. xiii + 224. $16.50, cloth; $4.95, paper.)

Political scientists who specialize in Southeast Asia have long faced a common problem: the lack of a single volume covering the current politics of the entire area and/or a series of volumes on the separate Southeast Asian nations under a broadly similar pattern of organization and analysis. Cornell University Press is attempting to remedy the situation by publishing a series, under the editorship of George McT. Kahin, on each of the larger Southeast Asian nations. The first book in the series, Josef Silverstein's *Burma,* is especially welcome. Burma has been the Southeast Asian nation most neglected by Western political scientists, primarily because the Burmese government has curtailed scholars' access to the country. Silverstein has expanded upon his earlier section on Burma in *Governments and Politics of Southeast Asia* (1964), also edited by George McT. Kahin, by providing more depth and placing greater emphasis on the period of military political dominance.

The book retains the essentially institutional approach found in the earlier essay, and this is both its major strength and weakness. Students of Burmese politics will appreciate the wealth of detail about the principal structures and institutions found in Burma. This is the first volume to discuss under one cover the myriad groups engaged in policy making and to provide a chronological framework for understanding the military period.

Silverstein's description of military rule in Burma between 1962 and 1976 comprises the core of the book. He takes the reader step by step through the convoluted maneuvers of Burma's military rulers as they shaped and reshaped the nation's political structures to retain and enhance their power. Silverstein's thesis that the period of military rule has been a time of stagnation is a thread woven throughout the book. He points out the military rulers' repeated failures in judgment, planning and execution over the past decade and a half. He places the principal blame for stagnation on General Ne Win and his followers and suggests that without a change in leadership, the flexibility and pragmatism needed to mobilize the nation's resources will not be forthcoming.

What is missing in this book is an overall orienting framework that goes beyond sequential analysis. In particular, the volume provides only a cursory cultural milieu for understanding the political behavior and dynamics of Burma's rulers. The result is a rather flat description that does little to highlight the uniquely Burmese character of the political system. Cultural values such as the Buddhist cosmology, *nat* worship, the concept of *a nade* (arising from status nonsymmetry where a subordinate is constrained directly confronting a superior), *pon* and *awza* (elements of power and authority unique to Burma), clearly have had an impact upon political behavior in Burma. In discussing the economic disintegration in Burma, for example, Silverstein pays too little attention to the point that Ne Win's government has operated within a traditional cultural context which has made modernization efforts both hesitant and difficult.

The author's choice to exclude peasant politics from his discussion no doubt stems from restricted access to local-level leaders. National politics in military-controlled Burma takes place primarily among the elites, yet a vibrant community of village headmen, monks, rural school teachers, rice entrepreneurs, and ethnic minority leaders participate in activities that affect a large number of the peasantry. By providing few insights into their role Silverstein misses an important dimension of Burmese politics.

Burma constitutes a rare example of a military government choosing socialist and neutralist policies. Yet this book does not adequately explain why the military government has pursued an active egalitarian socialism in the countryside despite the fact that the rulers had little to fear from the peasantry, but a great deal to fear from elites whose economic interests were being attacked. The socialist efforts of the Burmese military are counter to the usual economic policies of military rulers in non-Western societies. An analysis of the backgrounds of the military including their early training and socializing experiences might well help explain this anomaly.

Silverstein's splendid annotated bibliography includes both Burmese and Western sources. A number of charts and tables throughout the book add clarity to the analysis. The index is thorough. The high quality of Silverstein's *Burma* bodes well for the new Cornell University Press series on Southeast Asia.

CLARK D. NEHER

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Roots of German Nationalism. By Louis L. Snyder. (Bloomington: Indiana University Press, 1978. Pp. x + 309. $17.50.)

"It is the purpose of this book to throw light on various aspects of German nationalism by combining some of my articles and lectures with additional material written especially for this study." So sayeth the well-known author of this uneven compendium of 13 "chapters" or case studies, three-fourths of which are based on his own previously published articles or scholarly papers. Some are revised or adapted, others are reprints; some are of fairly recent origin, others were first published more than 25 years ago. Louis L. Snyder is a respected historian who has published at least four other books on nationalism, including two dealing specifically with German nationalism. Since the death of Hans Kohn in 1971, he has been probably the leading U.S. authority on the eighteenth- and nineteenth-century genesis of modern German nationalism. Re-treading a great deal of familiar ground and doing it in this particular format creates problems, however, including problems of coherence and integration, of repetition and representativeness. A brief exposition of the book's structure and approach may reveal this.

Snyder opens with a case study of Friedrich List and his work toward a German *Zollverein* as an example of German economic nationalism; follows with an analysis of the Grimm brothers' fairy tales to illustrate the cultural component of nationalism; then takes up the process of political consolidation leading to the founding of the Empire in 1870 (this is the only chapter Snyder himself singles out as having been written specifically for this book). So far, so good. We now proceed with four seemingly unrelated aspects of Bismarck's policy and personality: German colonization of Southwest Africa, the U.S.-German battle over pork imports (1879–1891), Bismarck's petty refusal to let his son marry the woman of his choice, and his pathological hatred of Edward Lasker, the cofounder of the German National Liberal Party. Next, German nationalism in the Weimar period is somewhat inexplicably exemplified by a detailed study of the pernicious and myth-creating influence of one Karl Goetz, a satirical medalist. Two subsequent chapters on the nationalism of Hitler and the Third Reich are followed by an essay on historian Hans Kohn and an examination of what light recent ethological studies (Lorenz, Ardrey, Morris) could shed on the question of innate German aggressiveness. A brief survey of post-1945 nationalism in East and West Germany concludes the book.

What does it add up to? Even the imposition of a chronological framework and deliberate efforts to create linkages can not quite overcome the reader's doubts. Is it defensible or helpful for our understanding of German nationalism, for example, to discuss Prussia's wars of national unification and the creation of the Empire in three pages, while devoting an entire chapter each to the trichinosis of American pork or to German acquisition of the Bay of Angra Pequena? Are there no better manifestations of Bismarck's authoritarianism than his distaste for son Herbert's bride-to-be? Is the medallion iconography of Goetz really the most convincing medium for conveying the cauldron of Weimar right-wing extremist *ressentiments?* These and other critical questions lost some of their validity if the book is accepted for what it is: a convenient collection of largely disparate essays, loosely related to a central theme. Thus viewed as an anthology, the individual pieces can be appreciated on their own merits. And most of them are very good, indeed: the essay on Hans Kohn, for example, is elegant and delightful. The author's style is lively and readable throughout, his judgment fair, reasonable and balanced. The continued use and justification of the concept of national character as a meaningful category of operational analysis, albeit with suitable qualifications, will no doubt antagonize many political scientists. Moreover, the discussion of postwar German nationalism in the Federal Republic is disappointing for students of contemporary German politics. In particular, Snyder's idea of a new West German "defensive re-nationalism" as a benign or desirable form of nationalism requires much more elaboration and clarification than it has received.

RICHARD H. BALD

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Communist Regimes in Eastern Europe. Third Edition. By Richard F. Staar. Stanford, Calif.: Hoover Institution Press, 1977. Pp. 302. Price not given.)

The third edition of *Communist Regimes in Eastern Europe* (the first and second editions came out in 1967 and 1971, respectively) attempts, but falls far short of fulfilling the increasingly serious need in East European studies for an up-to-date elementary survey of the area. Richard Staar takes a country-by-country approach to his subject, devoting a chapter to each of the eight countries of the region, and supplements these by three chapters on the Warsaw Treaty Organization (WTO), the Council for Mutual Economic Assistance (CMEA), and intrabloc relations. The book contains a useful bibliography, an index of names, and valuable tables on political, economic, and social topics, as well as organizational charts of the WTO and CMEA.

The book suffers from two major flaws, one of organization and the other of the intrusion of ideological predispositions into the analysis. Staar's arrangement of his material, virtually unchanged since the second edition, includes discussions of history, government structure, the Communist party, and domestic and foreign policy, but his presentation of these subjects is so haphazard and arbitrary as to be of almost no comparative value. The book's usefulness as a reference volume is severely diminished by the absence of a reasonably set structure for each chapter, and its utility for teaching purposes is almost nil unless one is inclined to take a country-per-class approach. The lack of a subject index compounds both of these problems.

Staar's unfortunate tendency to editorialize further detracts from the value of the text, especially as a teaching tool. References to "communist totalitarian states" (p. 6), characterizations of the Communist party as " 'a power unlimited by nothing, by no law, based on violence' " (p. 32), constant citations of economic, political, and social failures without attempting to describe successes, the use of outdated terminology such as "satellite" and "orbit"—all combine to make the book read more like a polemic than an example of scholarship. In his chapter on Yugoslavia, Staar even goes so far as to state that "there can be no question about Yugoslavia's strong preference for alliance with the Soviet bloc," as if the history of Soviet-Yugoslav relations over the past 30 years did not exist. Staar's attitude toward his subject is best summed up in the book's closing paragraph:

> The common desire on the part of all communist regimes is to remain in power, and they do not now and never have held this position by the will of the people they rule. . . . Perhaps the only hope for Eastern Europe must be sought in the long-range process governing the development of human society, which in fact represents communism's invincible enemy (p. 280).

The book is not totally without merit, however. There is much information of value here despite the frequent overstatements and outdated concepts, especially in the statistical displays. Ultimately, however, the volume's main redeeming feature is that it is the only current basic survey work available. It is a commentary on the state of the field that this is the case.

JAMES P. MCGREGOR

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Housing the Urban Poor in Africa: Policy, Politics, and Bureaucracy in Mombasa. By Richard E. Stren. (Berkeley: Institute of International Studies, 1978. Pp. xii + 330. $5.95.)

In emphasizing the elements of decisional choice in relation to political and economic resources, policy analysis seems an appropriate analytical tool for observers of Third World politics. Students of public policy can approach their subject in a variety of ways; they can focus upon the effects of different decision rules, cost-benefit or cost-effectiveness considerations, or the interactions between the political system and the environment. Irrespective of which approach is used, however, policy analysts share a common concern over applying "rational" decisional practices to government efforts to extract resources, to establish priorities on the allocation of public resources among competing objectives, and to set regulations on enterprise and activities. Such a line of analysis, with its stress upon the government as an action agency to be harnessed for the achievement of collective goals, orients the "new" political economy toward a problem-solving perspective.

Richard Stren's study of housing policy and politics in Monbasa, Kenya appears at a timely juncture. Our interest in applying policy analysis to Third World problems is increasing rapidly, making us keenly aware of our information gaps on such important issues. If the book seems somewhat lacking in focus—largely, I feel, because Stren never really delineates his central problem clearly and systematically—the data presented remains useful nonetheless.

Stren is at his best in probing the contradictions in urban housing policies over an extended time period. During the colonial years, public authority expenditures on housing were minimal, and the African population, constrained by low wages, had little to spend upon permanent dwellings for themselves. In the 1920s, a debate over planning priorities brought deep differences between Monbasa's Medical and Survey Departments into the open on the relative advantages of incremental and comprehensive change. Both sides agreed on the urgent need for improved housing, but they diverged noticeably when applying a cost-benefit calculus to the issue of urban redevelopment. Similarly, the use of a low-cost village layout system and local Swahili-type housing to deal with the challenge of low-income housing provoked sharp criticism on grounds of health and sanitation. In general, the incrementalists triumphed and, partly as a result, the housing situation worsened seriously by the end of World War II.

The post-war period, and the emergence of labor (and subsequently, political unrest, brought a new sense of governmental urgency to the problem of urban housing. The colonial regime responded by pushing the construction of large public housing estates, seeking both to promote a stable urban population and to reduce political grievances. The advent of independence brought little basic change in the broad outline of this policy. A National Housing Corporation was set up in 1967 and given a strong mandate to cope with the pressing housing needs of the country. Even though the construction of public housing units increased rapidly, Stren expresses serious reservations about how such a policy was put into effect. First, he argues that a class bias was evident in the way that services and benefits were extended to middle- and upper-income groups; and second, he contends that a regional bias was apparent in the emphasis upon prestige projects in the Nairobi area. Although regulatory policies have generally preserved the relative position of the lower-income groups in (relatively less-advantaged) Mombasa, "the net effect of public housing projects since Independence has clearly been to divert resources to the most privileged groups in the formal sector of the economy" (p. 284). Moreover, the expenditure of scarce resources on high-cost housing estates in preference to Swahili "slum" areas has reinforced the position of non-coastal Africans employed by large private and public corporations in this port city. In brief, economic growth has occurred but in such a manner that equity objectives seem largely overlooked.

In contrast to much comparative analysis, this book tends to be short on theory and long on empirical information. It presents readers with a great deal of interesting data, but leaves us with much of the task of tying together the various strands. Those prepared to make the effort of systematization will find this volume worthwhile reading.

DONALD ROTHCHILD

University of California, Davis

Tribalisme et parti unique en Afrique noire: esquisse d'une théorie générale de l'intégration nationale. By Lanciné Sylla. (Paris: Presses de la Fondation nationale des sciences politiques et Université Nationale de Côte d'Ivoire, 1977. Pp. 392. FF 95, paper.)

The African single-party system has been the subject of numerous publications, but in spite of the number of works now available, this new analysis of the "parti unique" by Lanciné Sylla of the University of Ivory Coast, is a valuable contribution. The author provides fresh insights and a breadth of coverage that makes this volume of use far beyond the small circle of students of African politics. He uses the contributions of the major writers in comparative politics and political development as well as most of the important African case studies. The works of French, British and American scholars are mined for ideas.

The first half of the book is a historical analysis of the growth of tribalism from the precolonial period to the present. Sylla emphasizes the effects upon this growth of the slave trade, of colonialism, of economic underdevelopment and differential rates of economic growth in various regions, and of universal suffrage. Quite correctly, Sylla notes that "tribalism" is the African variant of the worldwide problem of pluralism and he suggests that if properly considered, tribalism is of the same essence of patriotism and nationalism.

The second half of this book is an analysis of the origins, development, and inevitability of the one-party system, the "parti unique," in Africa. The parti unique successfully monopolizes the expression of the national political will (p. 26), but its existence does not necessarily mean that no other parties exist. The parti unique may be a single party or an overwhelmingly dominant party.

The parti unique as it now exists in Africa is not a solution to the problem of tribalism for it is a device by which a small group of leaders exploit the masses and a facade for ethnic domination. Instability occurs as ethnic groups compete for domination in a zero-sum game. This competition often leads to military involvement, another facet of tribalism.

Sylla views the parti unique as so African in nature, so appropriate to African political culture, that it is the means of rule most appropriate for Africa. But it will only operate effectively in an Africa without borders. The unity of African political culture will be the basis of the success of the parti unique in a united Africa, a unity of a common destiny and a common culture. States will continue to exist, he argues, but with flexible borders that will mitigate the antagonisms of tribalism. The meaning of tribal identity will lose political significance in the larger entity of Africa. This is a weak point in the rigor of Sylla's analysis. An Africa without borders is a concept that has refused to be measured. Sylla fails us, for we are given no insights as to either the manner in which "l'Afrique sans frontières" will evolve from the Africa of today or as to steps to take to encourage such an evolution.

Sylla's thoughts are based on a comparative analysis of four states (Ghana, the Ivory Coast, Senegal and Uganda) which were selected on the basis of two criteria—their roles as historical pacesetters in African political development and the wide diversity within each of precolonial political cultures. These criteria are not sufficient to exclude other choices and one might argue that the sample he has selected is not representative. Too, other scholars might wish to select different criteria.

Of course, in a book that covers so much political history and so much of recent political science theory, one can find many points to dispute. For examples, Sylla argues that it is certain that parliamentary democracy encourages multipartyism, that tribalism *appeared* to die away during the colonial period, and that African leaders are somehow unlike leaders elsewhere in their inability to share power. Also, there is an occasional datedness or incompleteness, as when he refers—in the present tense—to ethnic domination in Nigeria's three regions or in his tendency to view the political history of francophone Cameroon as the political history of all of Cameroon.

The major value of this book is the author's ability to make a coherent whole of so many different aspects of history and political science. Sylla is not the first scholar to see the various relationships he discusses, but the clearness of his statement, the logic of his arguments, and the comprehensiveness of his view make reading this book so worthwhile.

MARK W. DELANCEY

University of South Carolina

Marxism and the Metropolis: New Perspectives in Urban Political Economy. Edited by William K. Tabb and Larry Sawers. (New York: Oxford University Press, 1978. Pp. viii + 376. $10.00, cloth; $5.50, paper.)

Contemporary Marxist students of urban processes have had to approach their task without the benefit of specific groundwork laid by the masters: neither Marx himself nor Engels was much interested in cities per se. Thus, the central themes of Marxist urban analysis are only now being identified and refined. However, American practitioners of this approach, if we are to judge by the essays in the present volume, have broken no new ground here, choosing instead to follow recent trails established by Europeans such as David Harvey and Manuel Castells.

At present, the overriding concern of Marxist urban analysis appears to involve the shaping and consumption of urban space. The Marxist contribution to the study of the city is the fashioning, as David Harvey puts it, of a "geographical imagination," uniting such concepts as urban space, social ecology, and urban history with the engine of the class struggle. Thus the heart of Castells' *The Urban Question* (1972; translated 1977) is the examination of "the process of social production of the spatial forms of a society" (p. 19). And in this volume the editors suggest that recent struggles over urban space in American cities, manifested in the mobilization of neighborhoods against urban renewal, has led to their conviction that the configuration of urban geography is a product of the class struggle.

The 12 essays in this collection cover a variety of topics: American suburbanization, the fiscal crisis, the nature of the housing market, and, for comparative perspective, urbanization in Cuba, China, and the Soviet Union. While the quality of the collection is uneven, a certain coherence is established by the common theme of space.

David Gordon's novel essay on the relationship between the development of capitalism and the growth of the American metropolis exemplifies the concern over the forces which literally give shape to the city. Since industrial "accumulation" required a great labor pool, he writes, nineteenth-century capitalists located their factories in cities, particularly *big* cities. Gordon suggests that the greater impersonality of large (compared to small) cities and the physical isolation of workers in their neighborhoods permitted capitalists a greater measure of control over their work force. After 1880, however, the number of workers in big cities reached critical dimensions. Workers fed upon each others' discontent. Increasing labor unrest around the turn of the century led capitalists to move their plants out of the city to the suburbs in order to isolate their workers once again and reestablish control. Gordon rejects "orthodox" explanations for industrial dispersal—for example, transportation developments and shifts to land-intensive production methods—to argue that the search for ways of controlling workers is the major reason for the beginning of suburbanization on a grand scale. The lesson is that "capitalist spatial forms . . . develop at least partly to reproduce capitalist control, helping maintain the class relationships prevalent in capitalist societies" (pp. 26–27).

Two other essays on suburban development also adopt the view that to control the allocation and use of space is to control the society living on it. But unlike Gordon both authors write with a firmer sense of the cultural impetus behind suburban growth. Patrick Ashton sees suburbs as a product of capitalist wage differentials within the working class; thus suburban settlements serve as vehicles for specialized consumption expenditures and represent the spatial manifestation of social divisions created by capitalist society. Except for the fact that Ashton underplays purely ethnic and racial factors in the production of the metropolitan mosaic, his view is conventional enough. So too is Ann Markusen's: she argues that the calculated function of suburban governments is to insulate class consumption and capital from the costs of central city social expenses.

The power of capitalist interests in the class struggle is also used to explain the urban fiscal crisis and to elucidate the history of urban renewal. John Mollenkopf suggests, for example, that much of the current instability of urban politics may be traced back to class-based conflicts over the nature of urban redevelopment.

Curiously, the articles in the collection finally teach us very little that differs much from the conclusions of more conventional approaches. Most urbanists would agree with the writers in this volume, for example, that suburbs divorce fiscal resources from central city needs; that profit motives have historically superceded social welfare and planning criteria in urban development; and that the spillover effects on employment and tax revenues from renewal and subsidized economic development are generally overestimated by their proponents.

Where these scholars differ from what are labeled "orthodox" approaches (unfortunately illustrated in several of these essays by the social science of Edward Banfield and Charles Tiebout, scholars who do not in fact speak for a great number of non-Marxists in the profession) is in their analysis of motive forces and in the essential narrowness of their concerns.

All events are explained essentially in monocausal terms, each of which is a variation on the class struggle. Thus suburbs are a product of the search by capitalists for a way to isolate their workers. The roots of neighborhood mobilization "in almost every instance" are to be found in struggles against the ruling coalition over growth (p. 141). The New York fiscal crisis can be traced to the flight of capital from the city. The development of the housing mortgage system is a function of the need of the capitalist state for a social control device to impose on the working class. Such arguments do surely enlarge our understanding, but the single-minded form they take avoids a consideration finally of the more untidy, even accidental, complexity in social history.

Finally, in concentrating on the idea that urban spatial relations are a product of the class struggle, these essays ignore many of the issues that have engaged other scholars and would perhaps profit from Marxist scrutiny. The quality and delivery of urban services, the nature of urban community, the interior structure of neighborhoods, and race relations are all common concerns that do not appear in this volume.

PETER K. EISINGER

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The Production of Political Television. By Michael Tracey. (London: Routledge and Kegan Paul, 1978. Pp. xiii + 283. $10.00, paper.)

Michael Tracey begins, unnecessarily, by seeking to justify his book. He then devotes a laborious, lugubrious chapter to his conceptual framework. This contrasts the liberal democratic vision of the media as a fourth estate scrutinizing and criticizing the powers that be, with the class-based view of the media as an appendage of the state and commercial elite whose interests it serves. These chapters no doubt reflect the book's origin as a doctoral dissertation.

The introduction and conceptual framework are followed by excerpts from a series of interviews with producers of political television (news, public affairs, news magazines) purporting to explain the internal processes by which the programs are made.

Next come four narratively forceful case studies. An explanation of pro-government coverage by the BBC of the 1926 General Strike in Great Britain is based on archival material. From interviews and a reading of the public record, Tracey delineates the political forces behind the not entirely voluntary retirement of then Director General of the BBC Sir Hugh Greene in 1968. Similar research is used to analyze the "Yesterday's Men" incident in which the leader of the recently defeated Labour Party objected strenuously to his portrayal in the BBC program. The work was shown with some deletions, the head of current affairs programs was replaced, and innovative ways of treating politics assertedly inhibited. One of the program's producers is quoted as saying: "Better be safe than imaginative" (p. 201). In the final case study, Tracey recounts his observations of the coverage of the February 1974 General Election by an ATV (commercial television) regional station.

Tracey's conclusions are frequently, variously and vigorously expressed. In brief summary, he argues that political television in the United Kingdom suffers from limitations of time, finances and technology; debilitating legal restrictions (especially those relating to libel and contempt); informal governmental control; and the lack of any meaningful conception of the audience. It is characterized by an emphasis on form and pursuit of audience ratings (commercial imperatives) at the expense of content. And it functions within a straitjacket of conventional values about the nature of politics and news.

The result, for Tracey, "is essentially a process of discussion and critique within sanctioned areas" (p. 244). Worse, "rather than serving the 'electorate' or the 'people' as an information-starved collective entity . . . such television serves particular institutional and political interests" (p. 248).

Perhaps Tracey starts with preconceptions, perhaps he omits significant information to protect respondents; whatever the reasons, the book's data do not lead ineluctably to its conclusions. In their interviews, for example, producers stress their autonomy and frequent lack of constraints, the importance of their personal views of the subjects and styles of their programs. Nor do they display much passion for high ratings. Moreover, with the exception of the General Strike, the case studies can be read as documenting the difficulty politicians have in influencing the content of television—even when they do try.

My disquiet, even occasional disagreement, extend beyond interpretations of data; Tracey's conclusions seem to me sometimes to substitute assertion for evidence and logic, to be more reflexive than reflective. Two brief examples must suffice. He berates producers for lacking any conception of their audiences and relying instead on their own judgments. Yet there is no reason to expect such conceptions, should they develop, to produce more provocative and revealing programs. Indeed, they might well result in the kind of race for the ratings he deplores. Similarly, it may be true that television can only present the surface of a topic, but again such coverage need not necessarily be favorable to elites or (and this is quite a different matter) supportive of the status quo.

Nonetheless, Tracey's portrayal of political television in the United Kingdom as more a conduit for than a challenge to official rhetoric and perspectives is convincingly and forcefully drawn. To this observer of the American scene, the implications are doleful. For we in this country have scarcely attained the standards he criticizes.

Tracey has done us a service in exposing the roots of political television in his country. His English publisher has done us a disservice by permitting an abundance of typographical errors and split infinitives, endowing the book with unjustified margins and difficult-to-read type face and spacing. The book is unconscionably rife with stylistic infelicities, repetition and flatulence. All these remediable defects detract from the work's undeniable merits.

DAVID L. PALETZ

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The Military in Contemporary Soviet Politics: An Institutional Analysis. By Edward L. Warner III. (New York: Praeger, 1977. Pp. viii + 314. $19.95.)

A major problem with academic works on the Soviet military in general and party-army relations in particular is that they tend to assume that the reader is already familiar with the complexities of military and party structures. Unfortunately, this knowledge is often lacking. And without a basic knowledge of the organizational structures upon which party-army relations are based, at least in theory, the non-specialist is unlikely to gain even a basic understanding of the dynamics of party-army relations. Edward Warner's study deals effectively with this problem by providing a useful description of military structures in both the governmental and party bureaucracies.

In an attempt to overcome what Warner sees as a failure by Western specialists to conceptualize the role of the Soviet military within the Soviet political system, Warner attempts to combine two different conceptual approaches; those of interest group theory and bureaucratic theory. He then tries to apply this conceptual framework within an institutional setting by focusing on a number of Soviet military policy issues. Unfortunately, his attempt to apply this "interest group/bureaucratic" model is unsuccessful.

To begin with, bureaucratic theory, as Warner defines it, is intimately tied to interest group theory and is subject to many of the same criticisms. For example, in presenting the bureaucratic component of his model, Warner argues that bureaucracies are distinctive institutions with discernible interests and views, and a desire to maximize functional group interests. These criteria are also used by scholars who have viewed the Soviet military as an interest group.

To substantiate his interest group-bureaucratic approach, Warner argues that the military is an identifiable institution possessing its own ideology and policy preferences. To prove his point, however, he would have to show that the military's policy preferences are significantly different from those favored by other segments of the party leadership. In fact, the party leadership may also favor the same goals. After all, as has been pointed out on more than one occasion, civilians sometimes have a tendency to be more hard-line and Hobbesian on such questions than soldiers. Unfortunately, Warner fails to show that significant intergroup differences exist.

In fact, Warner himself discusses a number of cases which show that the military itself may be deeply split on basic issues such as doctrine or force structure, thereby raising serious questions concerning the utility of his conceptual framework. His discussion of the clash between different segments of the armed forces over Khrushchev's plan to cut drastically Soviet ground forces in the early sixties in favor of greater reliance on missiles is a case in point.

This theoretical confusion over the role of the military as an interest group/bureaucratic structure weakens Warner's otherwise interesting discussion of topics such as Soviet strategic thought, force posture, and arms control. Warner provides interesting material on these topics, but the reader is never sure of their significance for the overall development of party-army relations in the USSR.

In my opinion, much of the theoretical confusion in this work could have been avoided had the author dealt with the discussion carried on in recent years over the applicability of the interest group model to civil-military relations in the USSR. In this regard, I was surprised to note that Warner does cite the theoretical work done by William E. Odom, who argues that there are no vertical cleavages between the armed forces and the party.

It is vital in studies of this type that the theoretical framework employed is consistent with the data analyzed. Either the party and the army are distinctive institutions with different interests as Warner argues theoretically, or they are not. If the latter is true, then, one is forced to conclude with Roman Kolkowicz that party-army relations are marked by ongoing conflict between these institutions over party attempts to control the military. This conceptual approach, however, is inconsistent with Warner's findings. His evidence suggests that conflict will generally cut across institutional lines, as Odom argues. Careful attention to the arguments presented by Odom (in a work first published in 1973) would have forced Warner systematically to come to grips with these basic theoretical questions. This in turn might have helped him avoid the incompatibility between his conceptual framework and the data analyzed in this otherwise useful book.

DALE R. HERSPRING

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Politics in Postwar Japanese Society. By Joji Watanuki. (Tokyo: University of Tokyo Press, 1977. Pp. x + 171. $9.50.)

This collection of nine essays by one of Japan's leading social scientists is an important contribution to Japanese political studies. Although four of the nine chapters have been published elsewhere, five are basically new to Western readers. Additionally, through the comprehensive introduction and minor editing, the older writings have been skillfully updated and coordinated to form a reasonably cohesive volume.

Part 1 is the most unified of the three parts. The dominant theme is that Japan has developed a unique form of democracy. The uniqueness is due to a number of characteristics including: an externally imposed but liberal constitution; an elite ruling leadership composed of business, a dominant conservative party, and a strong bureaucracy; an extremely strong economy growing at an annual rate of 10 percent for nearly two decades; no external or internal security threats of note; and the fact that Japan is the only practicing modern democracy in East Asia.

A major conflict in the system is that between the old values of family, lifetime employment, and personal loyalty, and the newer industrial and post-industrial values of life and health, environmental protection, expanding income, and inflation controls. Although asserting that this conflict "makes the prediction of the future of Japanese politics very uncertain" (p. 58), the author is willing to conclude "that Japan is very unlikely to abandon its democratic institutions or negate its democratic values" (p. 62). Dahl's model of polyarchy is the source of this conclusion—polyarchy being made functionally possible by a combination of the postwar democratic institutional reforms of the constitution, a continuation of the [pre-military] prewar liberalism, and the recent expansion of citizen participation—all operating in a traditional environment of accommodation (consensus?) rather than confrontation.

Part 2 is more disparate. It includes a short but highly theoretical and complex analysis of the relationship between social structure and political participation, a long, dated (1950s), and previously published study of voting behavior, and a very brief essay on intellectual participation in foreign policy decision making. The main strength of the first two essays is the successful attempt to apply cross-national methods and data to hypotheses on Japanese political behavior. This is the activity which has placed Joji Watanuki in the company of the world's leading comparative political scientists.

The new material comprising chapter four is another excellent example of Watanuki's comparative skills. He presents three propositions about Japanese political participation, the thrust of which are that political *participation* is higher in rural areas while political *interest* is higher in urban areas—contrary to the propositions on Western democracies put forth by Norman Nie and associates in 1969. The propositions are tested with six variables of participation which have been derived from the Cross-National Research Program in Social and Political Change (Verba et al.)—leading to results showing "an amazing difference between Japan and other countries" and "confirm[ing] the propositions suggested. . ." (pp. 69–70). This leads him to call "special attention from the comparative point of view" to the existence "of a reverse relationship between rural-urban residence and political participation" (p. 74). Three causal reasons are cited for this "peculiar Japanese pattern," two of which, being peculiar experiences, are virtually impossible to replicate, leading the author to admit that the implications "for a general theory of political participation are not clear" (p. 75).

In spite of the dated statistics and information, the book's longest essay (comprising chapter 5) remains among the best explanations for voting behavior in Japan. Watanuki lists four comparative voting behavior patterns and then offers a series of explanations for each, utilizing survey data from a variety of sources. As in his other explanations, a partial explanation for differences are found in Japan's traditions—what he terms "cultural politics," wherein cleavages derived from value system differences have more political effect than "cleavages caused by economic, or status, differences" (p. 92). These culturally based cleavages when superimposed on intense economic discontent or status resentment, can result in "extreme political conflict, emotional and generalized, . . . and change in total social structure can occur" (p. 98).

Part 3 is the least cohesive, with chapters 7 and 8 being closely related essays on the theme of nation-building in Asia, while chapter 9 relates to Japanese attitudes "to the rest of the world"—hardly the same theme. Chapter 9, in fact, seems to fit better with Part 2. The primary objective of the opinion poll upon which it is based was to ascertain Japanese "international attitudes" (i.e., Japan's relations with the U.S.A., China, the Soviet Union, Korea, and other Asian nations, plus the security of Japan). However, the data collected on the independent and intervening variables influencing those attitudes is invaluable for students of domestic politics as well. In addition to the factors of sex, age, education and party preference, there are measurements of attitude towards the constitution, the emperor, nuclear armaments, and Japan's security. Watanuki's primary conclusions are "that domestic liberalism and international pacifism and neutralism will continue or even grow steadily in Japan. . ." (p. 153). A secondary conclusion is that the United States is not yet sensitive enough to the domestic factors behind Japan's foreign policy, leading to possible errors in its own foreign policy actions.

Finally, on the theme of nation-building, Watanuki shows the importance of building theory on three levels—domestic, regional, and international. He explores the question of the uneven development of China, Korea, and Japan and concludes that in spite of similarities among the three countries, key differences in leader-follower ties of loyalty and center-region relationships are important explanatory factors. He also expresses concern about the practice in comparative studies to stress the larger nations of a region while ignoring the politics of the smaller ones, from which, he feels, much can be learned.

Watanuki then follows his own advice, making a comparison at the regional level by seeking to elaborate on the difference in modernization patterns in Japan and Korea. The key variable, he believes, is the "linkages *between* the center and the periphery" which was lacking in the highly centralized Korean Yi dynasty but which developed in the "centralizing and decentralizing forces" of pre-modern Japan. Japan was able to use the traditional linkages to successfully create a modern state, while Korea succumbed to external invasion.

The real value of Watanuki's contributions here and elsewhere is that he is a genuine practitioner of the art and science of comparative politics, combining his knowledge of Japan with his Western training for the benefit of both Japan specialists and comparative generalists. The Japan Foundation deserves credit for assisting in the publication of this valuable collection of essays.

LEE W. FARNSWORTH

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Socialist Society and Free Enterprise Politics: A Study of Voluntary Associations in Urban India. By Robert G. Wirsing. (Durham, N.C.: Carolina Academic Press, 1977. Pp. xiv + 214. $11.00.)

A one-line summary of this review would read: sensitive, rich, and precise in description, truncated as to theory.

In 1969–1970, Wirsing closely observed local politics and the city election in Nagpur, an interior Indian city the size of Milwaukee. There was a lot to observe. There were 534 candidates, of whom he interviewed 136, randomly chosen. From this and much other information, Wirsing draws as detailed, fascinating, and convincing an account of Indian ward politics as I can ever recall reading. The scene is full of energy, ingenuity, commitment to the game.

This is, however, incidental to the book's purpose. Wirsing wants to show us the large part played by organizations of a particular kind in Nagpur politics. Chief among these are handloom weavers' cooperatives, housing cooperatives, and school management societies. These categories account for 787 associations in the city, leaving aside more exotic types such as wrestler's clubs and temple trusts. In contrast to Nagpur political parties which are weak, divided and shifting, the associations are emerging as effective instruments of aggregation. Of his candidate sample, Wirsing shows that two-thirds hold executive positions in such associations, high vote-getters even more so. (He makes little further use of his sample interviews to test propositions; evidently their unstructured format would not bear coding.)

Additional weighty evidence comes from a series of election cases which show the varied ways in which these associations are used to gain voter support, and under what conditions. Describing critical cases is Wirsing's principal method of demonstration.

In his introduction, Wirsing raises three theoretical questions (pp. 7–14). One assumes data will be brought to bear on them, but this turns out to be true only of the first question: (1) Is competitive group politics deepening or weaving new ties across India's divisions of religion, caste, and language? The highly relevant case Wirsing develops is that of the national Jana Saugh [Jana Sangh] party, operating in the city. Born of urges to revive, unify, and protect Hinduism, urges especially keen in this very region, the Jana Sangh has, in fact, begun systematically to push its candidates toward entrepreneurial roles in organizations of a wholly secular kind. This could represent boring from within; the rich circumstantial evidence of this book does not admit of much belief in that possibility.

The other two theoretical questions, however, are left pretty much hanging: (2) Are traditional attitudes or roles of deference and dependence giving way in city politics to reciprocity between leaders and their voter clienteles? Wirsing thinks so. But his evidence contains a serious ambiguity. Most of the voluntary organizations he studies exhibited a "glaring dependence" on government, often a heavy financial subsidy. And the more electorally successful candidates are involved in the more heavily subsidized types of organizations. One can't help wondering, unguided even by the adducing of any critical cases, in which direction the predominant influence flows.

(3) The final theoretical issue is partly related to the second. Granting that the government's policy of sharing responsibility for implementing development programs (e.g., propping up the handloom industry, or managing elementary schools) may yield political values in the diffusion of power, is there too high an economic cost in terms of program achievements, or nonachievements? Wirsing cites the "soft state" critics, Gunnar Myrdal at their head, who answer: yes.

Even to approach an answer, one would have to specify, from some political economy perspective, what the economic potentials are in Nagpur, what the alternative strategies for developing them might be, and which option the Indian strategy represents. There is no such perspective in the book. This hobbles Wirsing in making the theoretical clarifications he is entitled to draw from his findings. Two illustrations follow.

At one point he considers James C. Scott's concept of corruption in developing nations as a way of interpreting the involvement of private organizations in the implementation of government programs. Wirsing in the end rejects the interpretation. He shows clearly that it does not fit politically. Scott's model (in his 1969 *American Political Science Review* article, not in his later book which Wirsing does not use) was drawn from the American city political machine. That monopolized power; Nagpur politics is fiercely competitive. Is there not an equally fundamental difference on the economic side which Wirsing misses? Monopoly of power in the American city machine was sustained by a symbiotic relation with an aggressive profit-seeking business sector, which used its government connection to thwart the competition to which it was presumably accountable. But the competitive market, populated by firms making profits, is as conspicuously absent from Nagpur's scene as a competitive politics is present. That is the meaning of the book's otherwise quite misleading title.

Whether economic enterprise might grow were government tutelage removed, whether India has at its disposal any more centralized, managerial way of doing such jobs as developing city housing sites, making a living by hand weaving, etc.—these questions are quite outside the book. Wirsing cannot therefore really exploit what he learned in Nagpur to clarify the question which in the end troubled him most. For he wrote during the recent emergency, when Mrs. Gandhi claimed that to solve its economic problems, India had to sacrifice just such disorderly, energetic competition in politics as Wirsing describes.

Theory is truncated in this book. But its description, remember, is penetrating, readable, immaculately accurate.

HENRY C. HART

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France 1848–1945: Intellect, Taste and Anxiety, Vol. 2. By Theodore Zeldin. (Oxford: Clarendon Press, 1977. Pp. vii + 1202. $29.95.)

The Second Volume of Theodore Zeldin's *History of France* is, like the first volume, a bizarrely brilliant work, which is intended to be, will be, and should be infuriating to political scientists. Its stance is anti-theory, anti-models, anti-generalizations. To Zeldin, politics are not the activities people find the most meaningful. Political changes generally pass people by, having little impact on what they mostly care about: love, ambition, morals, taste, anxiety, hope, worry, boredom, birth, death, god.

The concern with politics, the state, the functioning of institutions, events, leaders, parties tell more to Zeldin about the observer of politics than about reality. The categories through which French politics have traditionally been analyzed—republicanism, socialism, opportunism, anti-clericalism—he regards as projections onto society. In each case, the variance within each category is almost as great as that between them. There is no socialism, but many socialisms, no republicanism, but many republicanisms, no bourgeoisie, but many types of bourgeois, no working class, but many types of working people, and so on. Since no group or movement has internal homogeneity, explanations attributing action to the working or interests of one or another of these categories is, for Zeldin, false.

It is certainly true that all such groups are heterogeneous. Zeldin's erudition is masterful in showing this. But he leads us to something akin to Xeno's paradox (if, in going from one point to another, you cover just half the distance, you never reach the end—unless you step the whole thing at once). In politics, even though groups are heterogeneous, action does occur, and it is groups, as well as individuals, who undertake them. Patterns and regularities of behavior do happen, even if those performing them think about them differently. France had high tariffs, conflict over schools, economic development, riots, revolts, Vichy and several republics. Each of these involved collective behaviors. Just why people cohered enough to do these particular things and not others is hard to tell from Zeldin's approach.

Zeldin wants us to think in new ways about old subjects, and to think about new subjects altogether. His chapters bear unconventional titles: "Good and Bad Taste," "Conformity and Superstition," "Fashion and Beauty," "Happiness and Humor," "Worry, Boredom and Hysteria." Familiar topics get grouped in startling ways with other topics under striking headings. The Army is treated under "Hierarchy and Violence," a chapter it shares with "Crime," "The Colonies," and "Servants." Economic history shows up in two chapters titled respectively "Technocracy" and "Gerontocracy." The penultimate chapter is called "Hypocrisy," where we find consideration of intellectuals, party politics, Communism, Maurras, Vichy, and the PPF.

As in the first volume, Zeldin's sensibility and capacity for work turn up countless nuggets: Those for whom French is not a mother tongue may take some satisfaction in knowing that had they lived in eighteenth-century Paris, they would not have had to master the present way of pronouncing "r," which is of recent origin; we learn what people thought of westerns—not much; they preferred history and murder mysteries on the screen. And we learn how Proust went about making sure he got attention for his work.

Among Zeldin's most valuable contributions are the many, splendid, short portraits of famous people and obscure ones, writers and politicians, artists and cooks. And, among the incredible range of material Zeldin explores, political scientists may take pleasure in finding the frequent and intelligent use of opinion polls on every conceivable subject, part of Zeldin's concern for the diversity of thought and feeling.

Both volumes are utterly indispensible, certainly to any student of French politics, but also to comparativists and other political scientists and historians willing to be provoked about what their subject matter is, whether it is important, and how it should be studied. Zeldin's work might best be comprehended as anthropology. Were he an anthropologist rather than an historian at Oxford, his work might be less controversial, but we might, alas, pay less attention to it.

PETER ALEXIS GOUREVITCH

McGill University

International Politics

The Decision to Divide Germany: American Foreign Policy in Transition. By John H. Backer. (Durham, N.C.: Duke University Press, 1978. Pp. x + 212. $9.95.)

John Backer has written a lucid account of how American foreign policy toward a unified Germany changed in an incremental fashion up to the Moscow Conference of 1947 when it became inevitable that the Western powers would go it alone. In addition, his study confirms some relatively recent insights by political scientists that grand designs are seldom implemented and that decisions are shaped by echoes of past mistakes, bureaucratic conflicts, and imperfect or selective use of facts in support of one's position. Furthermore, he gently attacks, with adequate documentation in his chosen period, the so-called traditional and revisionist schools of historiography. While he does not treat Soviet motives at length, Backer rightly condemns the all-too-often prevalent view that the USSR was hell-bent on world conquest and at the same time rejects as simpleminded the revisionist stances of Alperowitz and Williams who suggest an analytical framework in which the U.S. deserves blame for constructing a postwar imperial network.

For all but those thoroughly acquainted with his documentary sources, Backer's work will prove an interesting, well-written, and balanced account of one of the crucial decisions leading to the cold war. Though it purports to be based on recently available documents, the factual material presented (except for recent correspondence with the late Lucius Clay), was available as late as 1963. Furthermore, while I have no quarrel with Backer's use of the aforementioned political science "insights," in general our brethren are notoriously ignorant of the real world of foreign policy decision making. I remember joining highly placed bureaucratic colleagues in laughter at presentations by distinguished political scientists at a conference in 1967. Except for small powers who have no choice, such as the United States at its inception (thou shalt have "no entangling alliances"), or cases where a real statesman like Bismarck (under Wilhelm I) has policy firmly in his hands, the foreign policy of a nation is not made rationally. It grows, like cancer. Even such a dolt as Wilhelm II could say on learning of French mobilization in 1914, "if grandmother were alive, this never would have happened."

A nitpicking critic might fault Backer for not giving weight to domestic political factors in his fine concluding remarks, although they are treated earlier in the text. Furthermore, he has not cited Irving Janis, *Victims of Groupthink*, an excellent study of the social psychology of decision making, which might have improved his work. Although I reject for myself the label "revisionist," I find the Soviet perspective on American decision making not well represented. For example, the French ambassador to Moscow, Georges Catroux (cited in Backer's bibliography), was appalled at the timing of the announcement of the Truman Doctrine at the opening of the Moscow Conference. Further, although Backer claims that the Soviets violated the Potsdam Document on reparations from current production, this was not specifically excluded and the preamble calls for implementing the Yalta Agreements, where such reparations were specifically mentioned. Also he neglects the importance to the Soviets of later Western waffling on the Oder-Neisse lands, an agreement intimately connected with reparations concessions by the Soviets at Potsdam. The serious critic of U.S. motives might fault Backer for ending his study at 1947, and not 1948, because the policies inevitably led to the Berlin Blockade which was Stalin's last defense against what he perceived to be serious violations of international agreements by the U.S. and its allies. Only with the implementation of the Ost-Politik of Willy Brandt (which Nixon and Kissinger tried to hold up until the 1972 election!) could the Soviets rest easy over our German policy.

RICHARD D. HUGHES

California State University, Sacramento

Soldiers, Statesmen, and Cold War Crises. By Richard K. Betts. (Cambridge, Mass.: Harvard University Press, 1977. Pp. ix + 292. $15.00.)

Richard Betts' book raises four principal questions concerning the role of military advice in those cold war decisions in which the possible use of force was considered. What did military advisers recommend compared to civilian advisers? What effect did such advice have on presidential decisions? What accounted for differing advice from different military officials? How should a president choose and organize military advisers? Presidential sources are the usual congressional hearings, memoirs, secondary sources and some interviews. Although the research is extensive, there is nothing here that a student of the field has not already seen. Betts is not as selective as he might be in his citations. For example, Halberstam's journalistic and undocumented *The Best and The Brightest* is cited as though it were a scholarly documented work.

Overall, Betts' approach is very broad-brush and at times, such as when he discusses the Vietnam War, somewhat simplistic. Nevertheless, the book serves a useful purpose in bringing together a good deal of information concerning cold war decisions, although some judgments must be questioned. For example, the following:

> A few people's actions . . . inspire such awe and admiration that their opinions are persuasive on any subject of importance, and their reputations and appeal to public or legislative constituencies are so great that they are powers

> in their own right. Only two such charismatic soldiers appeared in the Cold War: Marshall and MacArthur (p. 174).

What is one to make of excluding general of the army and president of the United States Dwight Eisenhower from that list? Another example (p. 178), the author states that Admiral Radford was "tolerated" by the Eisenhower administration. He was in fact specifically selected because his views were harmonious with Ike's on the New Look strategy, and Radford had the personality to impose them on the other chiefs. Moreover, he has to this day been the most powerful chairman of the joint chiefs since the office was established.

Betts' overall conclusions are that the military has been neither more nor less aggressive than civilians on intervention decisions, but has been *more aggressive on subsequent tactical* decisions involving escalation once an intervention took place. He also concludes that when they opposed interventions the joint chiefs were more influential than when they supported an intervention. Further, Betts concludes that army chiefs of staff were the most cautious of the service chiefs, and chiefs of naval operations the most aggressive. An interesting book for those who already have background in the cold war period, it should be used with caution by beginners, because of the superficial nature of many of its judgments.

DOUGLAS KINNARD

University of Vermont

Foreign Affairs for New States: Some Questions of Credentials. By P. J. Boyce. (New York: St. Martin's Press, 1977. Pp. x + 298. $19.95.)

While any new work on the foreign affairs of newly independent states is a welcome addition to the literature, it is difficult to get a "handle" on this one. The table of contents alone gives one the first clue that this work will shift from generic processes to case studies. Chapters like "Foreign Policy, Diplomacy and the New State," "Sovereignty and the External Affairs Power," "Diplomacy, Law and International Disputes," which seem to be generic, are interspersed with "case study" chapters like "Papua New Guinea: An Australian Preparation" and "Two Southeast Asian Case Studies: Malaysia and Singapore." Yet even in a generic chapter like "Political Leadership and Foreign Policy Making" the subheadings suddenly shift from "generic" topics like: "Executive Control" and "Constitutional Limitations" (which in no way proport to be systematic, but rather represent the author's impressions) to "The Foreign Minister" (in which the author reports on Modelski's previously published quantitative analysis), to "Indonesian Counter Coup 1965" (in which he talks about the effects of the Indonesian coups on some Indonesian ambassadors). The author seems most familiar with Southeast Asian and Oceanic examples (followed by Commonwealth examples), as may be expected from an Australian.

While it is difficult to single out an entire chapter as being particularly informative or insightful, there are a number of isolated pieces in a number of chapters that are so.

Is the book supposed to be a guide for the foreign officers of new states? Certainly there are portions of many chapters which might be helpful to them. Is it supposed to be advice to governments about to become independent? They too might find it useful. Is it of use to scholars? Well, depending on what they have read previously, it could be. The author obviously does know a great deal about the organization and staffing of foreign offices in many of the newer states, but the manner in which the book is written makes the examples appear anecdotal rather than illustrative of trends, as I believe he intended.

Boyce is obviously very knowledgeable, but he has simply not presented his knowledge in a very organized manner. The fact that the concluding chapter is not quite three pages long says a great deal about the book. The entire work is disjointed and very much lacking in focus. I suspect that some reorganization and substantial editing could have made this a much better book than it is.

MARSHALL R. SINGER

University of Pittsburgh

The Future Global Challenge: A Predictive Study of World Security, 1977–1990. By Neville Brown. (London: Royal United Service Institute for Defense Studies, and New York: Crane, Russak, 1977. Pp. x + 402. £9.95.)

Neville Brown must be a stimulating conversationalist, or so one concludes from his published work. He has compelling observations to offer on a formidable array of topics and regions of the globe. His book is essentially a string of such conversational vignettes packaged under an imposing title and chapter headings—the result being, unfortunately, a work which advertises more than it delivers. Perhaps if he had selected a less pretentious title, or if in chapter 21, for example, he had resisted promising to show us "A Menacing Interaction of Trends," one could simply admire his style, erudition, and frequently sage counsel without expecting more in the way of integrated analysis.

In an integrated analysis there should be implicit or explicit criteria of selection, emphasis, and sequence of topics; and "conclusions" should follow from the body of the analysis. The fact that this book does not meet these standards is no reason to advise against reading it. But the reader should be forewarned–if only to avoid the exasperation I encountered in looking for, and failing to find, the integrating concepts–that this is hardly an analytically powerful book.

This having been said, I should like to recommend the book to anyone who has not been reading the international affairs sections of quality newspapers or magazines for the past several years (because of illness, imprisonment, or being too deeply involved in some micro research project). For here one will find informative, clearly written disquisitions on everything from urban problems to resource scarcities, to the problem of human rights in the Soviet Union, to NATO nuclear strategy. Neville Brown takes moderate and practical positions on the various issues, such as one might expect to find on the editorial pages of the *Times* (London or New York).

In short, the book might be placed on the reading list for a summer camp on world affairs for metallurgical engineers who want to broaden themselves. I do not feel it would be worth bringing to the attention of Brzezinski, or, for that matter, put on the required bibliography of a graduate seminar in international relations.

SEYOM BROWN

Carnegie Endowment for International Peace

The Dialectic of World Politics. By Silviu Brucan. (New York: Free Press, 1978. Pp. xii + 163. $13.95.)

State Systems: International Pluralism, Politics, and Culture. By Robert G. Wesson. (New York: Free Press, 1978. Pp. vii + 296. $14.95.)

Revolution of Being: A Latin American View of the Future. By Gustavo Lagos and Horacio H. Godoy. (New York: Free Press, 1977. Pp. xxvii + 226. $14.95.)

These are three visions of the future of the world political system. Curiously enough, the view of an unorthodox Rumanian Marxist, Silviu Brucan, is the most traditionalist in a Western sense. Robert Wesson, an American conservative, both discerns and laments the decline, and probably the demise, of the very nation-state that Brucan (p. 33) still sees as "the leading actor on the international scene." Gustavo Lagos and Horacio Godoy are romantic revolutionaries who bemoan the "anti-values" that predominate in the current world system (see Table 2–1, pp. 100–01) and offer a prospectus for breaking away from these in the next two decades, by violence if necessary. The Lagos-Godoy vision is both the most radical and the most precise; Wesson's is the most despairing; Brucan's is the most comfortable.

The Brucan argument may be the least inherently interesting to American political scientists and students of international relatons since it is intended primarily "to make up for a fundamental lacuna in Marxism": its "almost exclusive" focus on class conflicts and its related neglect of "ethnicity" as explanatory factors in international politics (p. viii). Brucan nevertheless knows a great deal about various schools of Western political analysis and his evaluations of these from a Marxist perspective may be somewhat refreshing and even challenging. The "Limited Growth" model of world futures, for example, is held to be too "conservative" in its assumption that everything in the world system will change except capitalism itself (p. 67). Other analyses are similarly disposed of.

The trouble with Brucan is that he has very little of rigorous substance to put in the place of these analyses. His own "theoretical model of projection into the future" (see Fig. 2, p. 142) is less than effectively operationalized and may border on the meaningless. In any event, Brucan foresees the "disintegration of the present world system" by the year 2000 and its displacement by a "world institution" (p. 152) whose genesis is scarcely argued and whose substance is not described at all. Vague as he is, it may be hard for Brucan to be wrong.

Wesson, on the other hand, would probably like to be wrong, but for ideological more than analytical reasons. The Wesson thesis is explicit: that "loose international system as against large empires, constitutional as against arbitrary government, and innovative against traditional societies . . . are closely related, and that, while cause and effect are inevitably reciprocal, the international situation can be usefully treated as a major independent variable" (p. 1). That "situation" is disappearing, however, and he wonders almost resignedly at last (pp. 17–18) "whether the [emerging] world system can function without the sharpness of dangerous competition and the plasticity of acceptable violence."

The proposition is clearly enough stated and the rest of the book purports to test it, but the proof is less than persuasive. There are two problems. One is the problem of inconvenient evidence, much of which Wesson is honest enough to adduce against himself if not always successful in explaining away. Ancient Egypt is frankly (and in my judgment, accurately) cited as the "best counterexample" of a creative imperial system which was that way virtually from the beginning, but "there must have been only slack or intermittently strong central leadership" in that system "and there is evidence [not offered, however] that this was the case" (p. 91).

Even if there is such evidence, there would still be the other problem of rigorous measurement. Wesson chides American students of international relations (p. 2) for ignoring their "chief potential data bank, . . . the historical record." But David Singer has not ignored it; Robert North has not. They have consulted it and measured it and used it in the derivation and testing of hypotheses about the behavior of states in the international system. Wesson has done no such thing. He may be right in his basic proposition that anarchy fosters creativity. But he has not tested it persuasively. When is a "system" anarchical and when is it not? Does anarchy have even correlates of creativity, let alone consequences? Like Brucan, Wesson has not operationalized his model.

Revolution of Being is the most thought-provoking of these three books. It is the latest in a projected series of regional perspectives in the World Order Models Project. Brucan (p. 141) briefly dismisses the whole project as too "normative." Project Director Saul Mendlovitz, in his introduction to this product of it, says that "utopia writing is an art, like good fiction or poetry" (p. xx). Nevertheless the Lagos-Godoy model is more rigorous than either Brucan's or Wesson's. The reader may be suspicious at first. There is a call for the creation of a new man, a reordering of society toward "humanist" values, that stirs fears of pipe-dreaming or, more importantly, doubts of any practical prescriptions for getting from here to there.

Those fears and doubts are short-lived. Latin Americans Lagos and Godoy perceive what Brucan says (p. 118) European peace researchers perceive, that the problems of world peace (and justice and welfare) are rooted in the system itself, and they would radically disagree with Wesson, that the "antivalues" of that system are worth preserving at all. But this is more than a vision or a polemic. It is a very explicit guide to the "revolution of being" that its title proclaims, and it pulls no punches.

Anybody can design a "preferred world." The problem always lies in the transition to it, especially in a relatively short space of time (in this analysis, the next two decades). How? It will take a "cultural revolution" (Ch. 5, Sec. 7), which is clearly meant in the Maoist sense, although Mao is not explicitly cited here and only rarely and somewhat incidentally later. This is, after all, a Latin American book. Its revolutionary hero, referred to and cited numerous times, is therefore, understandably enough, Che Guevara.

Lagos and Godoy do not advocate violence. But they foresee it, and for quite persuasive analytic reasons (Ch. 11, Sec. 2). Indeed, they caution explicitly that "as a general rule of action, violence should be avoided" (p. 184). But it will be necessary "in genuinely extreme cases" or "when all other means have been exhausted" (pp. 184–85). It is clear who the revolutionary heroes of this book are not: Mahatma Gandhi and Martin Luther King.

Of these three analyses, Brucan's predicts the least change by the year 2000: nation-states will still be significant; the East-West and North-South conflicts will still be with us, although they may have produced between them a new "world institution." Although Wesson hopes in the end (p. 265) that "humanity can learn, in effect, to compete on a higher level" than the nation-state, and thus preserve its creativity, he does not really believe that it can. Lagos and Godoy advocate and expect a radically different world that will have been brought about by the "struggle" of "a militant political humanism" (p. 98). Different readers will have different preferences. The Wessonites among us are probably already lost. But if we do not work quickly and skillfully to achieve or maintain Brucan's surprisingly conservative vision we will either make or get the Lagos-Godoy world "humanist" society, or we will get disaster beyond description. It will be a close race.

KEITH S. PETERSEN

North Carolina State University

The Geopolitics of Energy. By Melvin A. Conant and Fern Racine Gold. (Boulder, Colo.: Westview Press, 1978. Pp. xviii + 224. $20.00.)

This book is clearly intended for the intelligent layman and not as an academic study of the issues. Upon receiving the copy to be reviewed for this journal, one would outline one's expectations regarding the issues to be posed in a scholarly study with this title, the methods of analysis to be employed, and the type of inferences that could be drawn. Upon completing the first reading, one finds that none of one's expectations are borne out. But the book does convey a good deal of information of value to the general reader. To elaborate:

The vulnerability of consumer countries to supply interruptions or to restrictions in their access to raw materials is, undoubtedly, one of the most critical issues facing the industrialized world. Terms like "access" and "vulnerability" must, therefore, be carefully thought out and delineated. All the policy-related terms in this book are loosely defined, and there is little concern for maintaining internal consistency or for developing a more rigorous way of looking at (or measuring) the type, range, scope, and extent of the "vulnerability" that is so central to this book. Describing "access," as the authors have done, in terms of "adequacy of volume, continuities of flows, and capacity to pay" (pp. 7–8) is a step in the right direction. But nothing more is done.

Next, there is an apparent neglect of the recent literature on "geo-politics." There is not a single reference to the contemporary work in that area. (Not that I would argue that references in themselves are useful, but their absence does indicate a set of important issues which are not treated at all.)

Since there are, indeed, many discrepancies in existing estimates of supply and demand relationships with respect to oil, it is disappointing that the authors do not provide sources for many of their tables, thus again making it difficult to identify whose estimates they believe are more accurate. Note 1 for Chapter 1 indicates that the CIA is the source for most of the figures. If this is so, then a comparison of CIA estimates with others would be helpful.

The description of the international oil industry is much too sketchy not to have elicited a plea for further expansion of that section or, at least, greater clarity regarding those aspects of the industry's organization that are particularly relevant to "geopolitics."

The same type of observation can be made with respect to the review of coal and nuclear energy and the projected assessments from 1977 to 2000.

Most surprising, however, is the fact that there is no reference to any of the systematic analyses of the world energy situation, or of the world petroleum market, or the 12–14 very respectable (even sophisticated) oil models in the literature. It would be interesting, and revealing, to know the authors' views regarding the projections or forecasts as represented by Adelman, Pindyck, Eckbo, the authors of the FEA studies, Kalymon's earlier work, the World Bank models, and the ongoing work in the Political Science Department at M.I.T.

Part 2 of this book, entitled "Policy Options," is a brief, and sketchy, review of general issues influencing United States orientation toward energy policy. It takes a shotgun approach to the problems at hand and the reader is left puzzled as to the rationale for the review and for the selection of the issues raised.

The Epilogue is tight, well done, and worthy of attention. However, it really bears no direct connection to the rest of the book. Finally, it must be noted that the "financial dimensions" (p. 219) of the issues are treated in a page and a half.

In conclusion, this is a book that most properly belongs in the popular domain. It is useful for someone who knows absolutely nothing about the energy situation. It is too superficial and sketchy to be used as an informed text. One is left with the strong impression that the authors are more knowledgeable, more sophisticated, more insightful, and possess a far greater understanding and appreciation of the policy issues involved and of the underlying "realities"—however debatable their precise implications may be—than is reflected in this volume.

NAZLI CHOUCRI

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Governments Under Fire: Civil Conflict and Imperialism. By William Eckhardt and Christopher Young. (New Haven, Conn.: HRAF Press, 1977. Pp. 379. $16.00, cloth; $8.00, paper.)

Marxist theories of economic imperialism have frequently been used to analyze international conflict. The general claim has been that capitalism can survive only by increasing accumulation of wealth, with the consequence of increasing inequality of income within and between nations. Thus, it is concluded, wealthy nations fight wars to compete for trade advantages that increase exploitative relations between rich and poor countries. The authors of this study, both members of the Canadian Peace Research Institute, extend this analysis to the study of civil conflict.

The basic hypothesis of this study is that international conflict is instigated by the Right and leads to increasing equality. Domestic conflict, on the other hand, is a response by the Left to the above condition and thus leads to decreasing inequality which, in turn, leads to decreasing conflict. If these consequences of civil conflict do not occur, at least such conflict is more justifiable than that instigated by the Right.

This book contains a rather exhaustive review of empirical studies of civil conflict. The authors note the general finding that civil conflict tends to be much more violent in the poorer nations than in the richer ones, and they call attention to the fact that this finding has never used the theory of imperialism as an explanation. This study, however, has two serious flaws: (1) the theory of imperialism fails to provide a better scientific explanation of domestic conflict than competing theories, and (2) the research design includes some dubious measurement decisions.

The basic function of theory in scientific analysis is to generate testable hypotheses that reduce unexplained variance in the phenomenon under investigation. The theory of imperialism fails to do this. Competing theories do at least as well. In testing their hypotheses, the authors employ questionable measures. Political imperialism "was operationalized by the number of years a nation was independent and by the number of years its latest constitution had been in effect, . . . the assumption being that the more established nations included the colonial powers of the past" (pp. 106–07). This certainly appears to be a strange decision. There is a lengthy discussion on measurement of civil conflict which is concluded by the confession (p. 91) that their preferred scoring was "lost" in the computing process, so they used something else. Since both variables are central to their theory, one is uneasy about any findings that are based on such inappropriate decisions.

The study will be useful to those who are engaged in cross-national research of civil conflict primarily for its exhaustive reporting of statistical tests for measurement validity on other variables and factor analysis of publicly reported cross-national data. Other readers will find the text cumbersome and difficult to follow. The conclusions of the study, though, are for the "true believer" who is already convinced of the merits of Marxist theories of imperialism.

BETTY A. NESVOLD

San Diego State University

The Conduct of American Foreign Relations: The Other Side of Diplomacy. By Thomas H. Etzold. (New York: New Viewpoints, 1978. Pp. viii + 125. $5.95, paper.)

In this essay Thomas Etzold offers an analysis of the obstacles that impede efficient conduct of foreign relations by the traditional machinery of government. Focusing upon the Foreign Service and the Department of State, his work espouses the currently popular theme that both have "found themselves displaced in their own areas of competence and responsibility" (p. 3), i.e., lost their roles as primary makers of foreign policy. The author advances a variety of reasons for this perceived dilemma: the failure of the 1924 Rogers Act to curb foreign service elitism, even as it afforded job security and merit advancement; continuing faulty departmental organization despite repeated attempts at organizational reform; the rise of the military to a permanent position of dominance in foreign relations as a consequence of World War II; postwar proliferation of governmental agencies involved in foreign affairs; incompatibility between concepts of management and diplomacy; the alleged eclipse of the department by the Nixon-Kissinger National Security Council system; a collision between a surviving foreign service elitism and emerging egalitarianism.

The author concludes that the department officials and the diplomatic professonals must recognize that they conduct rather than make foreign policy, another way of saying that to think small is beautiful. Such a conclusion is shared by others. It was, after all, President Harry Truman who said, "I make foreign policy," an assertion which every one of his successors, except possibly Eisenhower, has reiterated and sought to prove by word and deed.

Etzold's study would be far stronger had he linked it to the literature of bureaucratic theory which emphasizes the implementation role of bureaucracy. Understanding the interactions through which policy is implemented is to comprehend the role of the Department of State and the Foreign Service in the foreign policy process. The Department of State and the Foreign Service are critical actors because they are charged by law with execution of foreign policy.

The volume benefits from an informative and engaging sketch of departmental history, organization and personalities from 1775 to the Rogers Act of 1924. It suffers, however, and sometimes heavily, from banality and inaccuracy. Lyndon Johnson rather than Richard Nixon assembled the NSC committee system; Nixon seized upon the existing apparat as a vehicle for regularizing and centralizing policy formulation in the White House. The author's descriptions of diplomatic mission organization are often inaccurate, e.g., the cultural attache is always, "not usually" (p. 90), an employee of the U.S. Information Agency (now the International Communication Agency), and the Public Affairs Officer is chief of the information and cultural effort and superviser of the cultural attaché at all posts, not only "smaller ones" (p. 90), except where the host country will not permit a full informational and cultural program.

Withal, this is a useful handbook for the uninitiated in spite of its limitations.

JOHN H. ESTERLINE

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Rich and Poor Nations in the World Economy. By Albert Fishlow, Carlos F. Díaz-Alejandro, Richard R. Fagen and Roger D. Hansen. (New York: McGraw-Hill, 1978. Pp. xii + 264. $10.95, cloth; $6.95, paper.)

Despite the increased academic attention which has recently been accorded to North-South issues, liberal scholars have on the whole been on the periphery of relevant theoretical work. This book partially remedies the situation through a set of essays devoted to the normative analysis of some of the issues surrounding the calls for a new international economic order. The bulk of this book is devoted to three essays by Albert Fishlow, Díaz-Alejandro and Richard Fagen. A brief introduction and concluding chapter are provided by Roger Hansen.

While recognizing the extent of the North-South economic gap, Fishlow believes that radical change would be considerably less useful than the consistent application of classical liberal principles. He musters some historical evidence suggesting that economic growth in developing nations has been positively correlated with the extent of their participation in international trade. If discriminatory practices concerning southern goods (as well as protectionist measures in general) were eliminated, lesser developed countries would benefit economically while industrialized nations would, in addition, display a previously lacking measure of ideological consistency.

Díaz-Alejandro's perspective is somewhat, although not radically, different. Well-versed in economic and international history, he places little credence in the benevolence of the market toward the economic underdog. While no *dependentista,* he argues for separating Third World economies from those of industrialized nations. The author advocates considerably less than the self-reliance desired by—say—Frank, Galtung, or even the Cocoyoc Declaration. It is, in fact, not so much self-reliance as selectivity and autonomous choice in their foreign economic policies that the author proposes for developing nations, the point being "to create an order that will accommodate a variety of experiences, particularly progressive changes in the South, and that will not be hostile to and sabotage Southern efforts to achieve greater economic autonomy as the old international economic order has often done."

Apart from their different prescriptions, Fishlow and Díaz-Alejandro differ in their appreciation of the political constraints facing the implementation of their proposals. Fishlow's belief in the current readiness of northern governments and of their publics to open their markets to the South is, to say the least, unpersuasive. The extent to which endogenous power structures would allow any benefits derived from the application of market principles to percolate down to the truly destitute segments of developing societies is also inadequately dealt with. Díaz-Alejandro, on the other hand, recognizes that dominant powers have never endorsed the right to selectivity by other nations; he also points out that most existing political arrangements suggest that internal inequities will not be attenuated by merely changing a country's foreign economic policies.

Richard Fagen, more than either of the two previous authors, focuses on the obstacles facing reforms designed to deal with relative inequality and absolute poverty in LDCs. There is, first of all, the obvious matter of the internal power structure of many Third World societies where the control of income is both extremely concentrated and coextensive with political, social and cultural influence. Under such circumstances, and in a market context, spontaneous reforms cannot be expected to occur. Only the state might be capable of acting against the grain of both social structure and market.

Unfortunately, growth-oriented policies typically lead the state to establish alliances with private capital from abroad, which severely limits its autonomy and ability to promote substantive and equity-oriented reforms. This observation, as well as the anticipated hindrance from the North, makes Fagen considerably pessimistic concerning future inequality and poverty in most southern societies.

In a final and brief essay, Roger Hansen advocates a transition from classical growth theory, with its emphasis on savings, investment and gross national product, to one emphasizing basic human needs. He recognizes that most of the financial support for such policies would have to come from the North and from southern elites, but believes (plausibly or not) that a willingness to underwrite such policies might be dictated by a calculation based on their own long-term self-interest.

To be useful, the essays should have clearly specified the desired equity goals *and* the nature of the political context within which they are to be achieved. While Fishlow and Hansen do quite well in terms of the first task, they perform the second much less well. It must be said, therefore, that Fagen, and to a certain extent Díaz-Alejandro, provide the more useful essays. On the whole, however, this is a well-written book which is valuable both for the myriad ideas it contains and for the importance of the issues which it addresses. Let us hope that it will contribute to establishing a fruitful dialogue between political scientists and economists on topics of immediate and global concern.

MIROSLAV NINCIC

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Humanitarian Politics: The International Committee of the Red Cross. By David P. Forsythe. (Baltimore and London: Johns Hopkins University Press, 1977. Pp. xii + 298. $17.95.)

The International Committee of the Red Cross (ICRC), an all-Swiss,Geneva-based organization, is the oldest component of the Red Cross Movement (which also comprises National Red Cross Societies and the League of Red Cross Societies, which is the federation of the national organizations). Yet the ICRC is an organization hardly known, generally confused with the Red Cross Movement, vaguely associated with prisoners of war, and little researched. Thus, David Forsythe's study—the first systematic analysis from a scholarly perspective (there do exist a number of ICRC "histories" or accounts by ICRC personnel)—represents an important contribution to the slowly emerging literature on non-governmental organizations (NGO's) in the human rights field. It provides a comprehensive yet analytically focused treatment of the work of this vital human rights NGO, concentrating on the ICRC's humanitarian politics: "the struggle to implement humanitarian values as official policy in the nations of the world" (p. 2).

The volume is distinguished by Forsythe's objectivity, no mean feat when studying a human rights NGO. Moreover, the ICRC is a particularly "discreet" body, not eager to provide access to outsiders or to explain itself to the public. Forsythe gained his intimate knowledge of the ICRC's policy-making processes and operations by becoming a consultant to the Henry Dunant Institute (the Red Cross Research Center in Geneva) and to the Joint Committee for the Reappraisal of the Red Cross; nonetheless, although he became an "insider," he resisted the frequently manifested tendency of insiders to venerate their institution. Forsythe accurately considers himself "a sympathetic critic" of the ICRC—"sympathetic toward what the ICRC tries to do for the individual around the world . . . but at times . . . critical of how the ICRC goes about its protection and assistance efforts" (p. xi).

In chapter 1, Forsythe elucidates the position and role of the ICRC in the Red Cross Movement. He focuses on the complexity of the ICRC's relations with National Red Cross Societies over which it has only the limited leverage of extending or denying them recognition; its relations with the League (the organization charged with dealing with natural disasters), made difficult both because of the breakdown of the premise that peace could be distinguished from war and by institutional jealously; and with the International Conference, theoretically the policy-making body of the Red Cross Movement but, in fact, largely symbolic in its importance. Chapter 2 sets out the ICRC's functions and philosophy. Since the former are poorly understood, much of the remainder of the book is devoted to an explication of these functions: the ICRC's ad hoc diplomacy on behalf of political prisoners; its ad hoc diplomacy with respect to hijackings, kidnapings and miscellaneous protection tasks; its role in the development of the law of armed conflict and human rights law generally; the ICRC's legal protection of combatants; and legal protection and assistance to civilians in time of war. There is also a chapter devoted to the internal workings of the ICRC—its organization, its personnel, its method of finance—about which almost nothing has been written because the ICRC regards its meetings and records as confidential. Most controversial, however, is the ICRC's philosophy and the methods which follow from that perspective: the ICRC's definition of "humanitarianism" and the politics it pursues to realize humanitarian values. Forsythe keeps this question in the forefront through his entire analysis.

In brief, the problem is this. The *Grundnorm* in Red Cross thinking is humanitarianism, or help to the individual in need. This help should be nondiscriminatory and it should occur "with the cooperation of public authorities" (p. 35). That is, the ICRC only proceeds with its protection or assistance functions on the basis of the consent of states. Moreover, it pursues a policy of discretion, only rarely engaging in public criticism of governments, even where it knows those governments are engaging in gross violations of human rights. Thus, the ICRC did not publish what it knew of the German concentration camps in order not to contravene the wishes of the Nazi government; it refused to testify before the Council of Europe on the torture of the Greek Junta; it made no public statement about My Lai or the "tiger cages" in South Vietnam; and it has made no sanctioning remarks about repression in Chile, or Uganda or South Africa. Where the ICRC gets access to political prisoners or POWs and finds human rights law being violated, it reports those violations only to the offending government with its recommendations for change. Nor is it concerned with exposing the causes of the violations or fundamentally altering government policy. Thus, by contrast with an organization like Amnesty International, which not only uses publicity as a moral weapon but which seeks the release of political prisoners, the ICRC is concerned only with improving the conditions of their imprisonment. Since to do this it needs access to the prisoners, and since access depends on governmental approval, the ICRC avoids offending public authorities. That is, the ICRC has tended to act on the "one more blanket theory." "As long as it can bring in one more blanket to someone in need . . . it will do the humanitarian good it can" (p. 65). Forsythe spotlights the moral problems of this approach but also underlines the fact that "whatever deficiencies exist . . . there is no other actor in world affairs that has done—or will be likely to do—what the Red Cross has done with regard to conflicts" (p. 53).

In his conclusions and postscript, Forsythe provides candid and perceptive comments about the ICRC's potentials and its weaknesses. An organization founded in 1859, the ICRC has had to adapt to the complex changes in the world milieu, especially the increasing importance of unconventional war, the decreasing importance of the West in the global power structure, and the proliferation of international institutions and interest in subjects of transnational import, including humanitarian affairs. Forsythe has no doubt that the ICRC has proved its usefulness in the past. He does, however, acknowledge that it has been "less than fully effective in the modern period" (p. 223) and that it will have to undergo critical reexmination if the ICRC is to become—as Forsythe thinks it should—"the leader of a global transnational movement active for the benefit of mankind."

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U.S. Intelligence and the Soviet Strategic Threat. By Lawrence Freedman. (Boulder, Colo.: Westview Press, 1977. Pp. xv + 235. $22.50.)

Lawrence Freedman starts from the premise that official perception of a Soviet strategic threat to the United States takes place in two stages: first, observed changes in the Soviet force structure and intelligence estimates and predictions of how such changes affect Soviet capabilities, and second, "a judgment on the character and sources of American strength and how that strength might be undermined. It is the intelligence community that is mainly responsible for the first of these estimates. The second judgement is made in the wider defense community and can be a contentious issue" (p. 5). To understand the relationship of threat assessments to strategic arms policy, "in addition to asking how intelligence estimates are constructed and how they are fed into the policy-making process, we need to examine the manner in which it is decided whether or not, and in what ways, the US would be vulnerable to specific developments in the Soviet Force structure" (p. 6).

Following a brief introduction which defines terms and sets forth this thesis, this extraordinarily lucid and well-organized study discusses the institutional history of the U.S. intelligence community and on the estimating process. The latter discussion explains the process by which U.S. intelligence assessments have been produced, and how the changes introduced by Henry Kissinger during the Nixon administration affected both the content and declining influence of such estimates. There follows a discussion of the key intelligence debates and estimates of the 1940s and 1950s with special attention paid to the so-called bomber, missile and ABM gaps as perceived during the period. The rest of the book "consists of a detailed study of the 'Minuteman vulnerability' issue as it developed through the 1960s and 1970s. This issue concerns the vulnerability of the US Minuteman inter-continental ballistic missile (ICBM) force to a first-strike from Soviet ICBMs. It simmered as an issue during the early and mid-1960's and then rather suddenly emerged as a major problem at the start of the Nixon Administration in 1969" (p. 6). Selection of this issue, which was at the heart of the SALT Agreements, allows the author "to assess in detail the performance of the US intelligence community in the face of the major strategic arms build-up of the Soviet Union which began in 1965 and has been progressing ever since" (p. 7). As a result of this survey Freedman concludes that the issue of Minuteman survivability remains unresolved after more than a decade of debate when, from 1969 on it was "accepted by all as the most serious potential source of instability in the strategic balance" (p. 182).

Two chapters, "Protecting Minuteman Through Salt" and "Preparing for the Threat: 1972–6," provide an excellent, detailed study of the intelligence estimates and public debate concerning the SALT Agreements through the beginning of the Carter administration.

Freedman's final chapter concludes that judgments on the Soviet " 'threat' depend not only on estimates of what the Soviet Union is attempting to do, but also on a sense of the vulnerabilities in the US military position, understood by reference to broader perspectives on the character and sources of strategic strength in the modern world. The process by which threats come to be perceived is a political process, in which the nature of the prevailing strategic doctrine in the US and attitudes toward the defense budget and arms control are as crucial as estimates on the evolving Soviet force structure" (p. 198).

This study is a refreshing change from the voluminous outpouring of "investigative" literature (official or otherwise) which deals mainly with covert or "political action" operations, an area which, as Freedman points out, has little or nothing to do with the heart of the intelligence function, the estimating process. It is also a welcome departure from self-serving memoirs of former CIA agents or administrators seeking to refurbish their own or agency images. After establishing the necessary factual base, Freedman expertly blends history and analysis in an account that is both lucidly written and compelling in its conclusions. The study is free of cold war cant and institutional pleading. In addition, there is an excellent index and a selected bibliography of government and open sources used. The book should serve as an invaluable sourcebook on the role of intelligence in strategic estimates and decision making for both the student, professional intelligence officers and policy makers.

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Marine Policy: A Comparative Approach. By John King Gamble, Jr. et al. (Lexington, Mass.: Lexington Books, D.C. Heath, 1977. Pp. xiii + 146. $15.00.)

Maritime affairs should be increasingly interesting to political scientists. More and more it embraces fields which interface not only with their discipline, but also with the broad range of their concerns.

A recent event in the Indian Ocean illustrates the many threads connecting maritime problems and political science. On 16 December 1977 the supertankers Venoil and Venpet collided off the coast of South Africa. The Venoil had a full cargo of oil, the Venpet was in ballast. Both tankers were of Liberian registry, had Chinese crews, but were owned by a subsidiary of a large American corporation. Two different South African government organizations took part in—and were in conflict over—the salvage operations of the two tankers; yet another South African organization dispatched specialized ships to contain the oil spill and prevent pollution; ships of still other countries participated in rescue efforts. Here were intermingled problems attractive to the political scientist: international law; international relations; environmental questions; public policy (U.S. national policy and the flag of convenience); problems of comparative government; government and technology.

This is the virtue of *Marine Policy.* It examines various attempts to deal with maritime policy and points out rather cogently that the treatment of marine policy "in isolation from other national policies" has been not too successful, if, indeed, it has not been self-defeating.

The resources of the oceans (marine animal life, off-shore oil, mineral nodules both in the deep sea and in lesser depths), merchant shipping, naval power, environmental concerns—this book deals with all of these and more so concisely that reviewing it is difficult. It is a tribute to the author and his contributors that had this book been prolix, it would have been easier to review.

Gamble's method is to employ five models or case histories, concerned with, respectively, Canada, the People's Republic of China, Iran, Nigeria, and the Philippines. Problems examined are as divergent as the Canadian Arctic Waters Pollution Prevention Act; maritime concerns of the Philippines related to its multi-island geographical nature; Persian involvement in the geopolitical ramifications of the Persian Gulf-Indian Ocean marine complex and in the interface of oil and maritime transportation; Nigerian attention to maritime affairs primarily only as they relate to the development of the country.

The Chinese case history is both interesting and timely. We are reminded that the PRC has a long coastline, offshore resources in oil and minerals, and the will to develop a modern merchant fleet and navy. As is pointed out, the PRC leadership appears to be quite well aware of the role of naval forces—and their supporting elements—in overall policy. Although not cited in *Marine Policy,* the evidence available in recent years strongly suggests that PRC naval policy is based on increasing naval capabilities vis-à-vis Russia. An example has been the charges made by China that the Russians have been given the right to make use of the naval and air base facilities in the Camranh Bay complex by the Vietnamese Communist regime.

The timely nature of the PRC concern with maritime affairs as brought out in the book is underscored by reports in the British press in February, 1978. These indicate that the PRC is negotiating with Great Britain concerning building both merchant ships and warships to be built in the United Kingdom for the Chinese account.

J. D. ATKINSON

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Verwaltete Aussenpolitik. By Helga Haftendorn, Wolf-Dieter Karl, Joachim Krause, and Lothar Wilker. (Cologne: Verlag Wissenschaft und Politik, 1978. Pp. 296. DM 32.)

Politikwissenschaft und Aussenpolitik in den USA. By Gebhard Schweigler. (Munich: R. Oldenbourg Verlag, 1977. Pp. 305. DM 46.)

Most American political scientists are familiar with the seminal works of authors such as Destler and Halperin on bureaucratic politics and foreign policy. We now have an equally important German volume on the bureaucratized foreign policy of the Federal Republic written by Helga Haftendorn, professor of international relations at the Free University of Berlin, and three colleagues.

Using several case studies in German security and detente policies, the authors describe the decision-making process. They conclude that, with the exception of the innovative *Ostpolitik* of 1968–72, German foreign policy making is heavily bureaucratized with minimal input by parliamentary or academic outsiders. The result is a powerful indictment of German foreign policy making: lack of coordination, lack of effective long-range planning, emphasis on conformity and routine, refuge in trivia, and inexperience in multilateral negotiations. These weaknesses are compensated for by Germany's smaller ability to play a world role.

For the American scholar, the case studies are of particular interest. The thorough analysis of the Brazilian nuclear deal (Lothar Wilker), the decision-making system of the integrated NATO bureaucracy in the mutual balanced force negotiations (Johannes Altmeppen), the development of the German position in the Helsinki European security talks (Rudiger Lentz), the German conventional arms transfer guidelines (Christian Loeck), and the bureaucratized relationship between the government and trade associations in the East-West trade negotiations (Arno Burzig) are especially well researched and well written.

In light of Haftendorn's critical research, the study by Gebhard Schweigler in the series issues by the research institute of the German Society for Foreign Policy (a German version of the Council on Foreign Relations) seems to have been written to influence German public opinion toward a broadening of the foreign policy-making process to include academicians, particularly political scientists. At a time when the reputation of German political science is at a low point, Schweigler presents a thorough overview of the relationship of political science to foreign policy in the United States. The goal of his study is not only to shed light on relationship between American political scientists and foreign policy but also to provide a handbook on American political science, at least as it relates to international affairs. As a handbook the lack of organization and the inclusion of some irrelevant and inaccurate detail (Fulbright started out as a law professor, not as an historian) makes the book less than useful.

Schweigler, who is the American specialist in the Bonn Institute, as well as the German representative of the prestigious German Marshall Fund, does succeed in tracing a somewhat cyclical relationship between political science and policy making. Looking at the effects of insiders and outsiders on the policy-making process, he notes that at the beginning of each administration there was an influx of academics, half of whom left after a time, disillusioned about their decision-making role, while the other half became bureaucratized. The disillusioned half then tried through publication and other public criticism to exert pressure to change the administration.

Knowledge may not be political power, but academic knowledge is not superfluous. Unfortunately, Schweigler spends too much time examining European and other major policy issues—had he looked at more specialized areas of policy such as Law of the Sea questions or the U.S. relationship to specialized international agencies he would have found a much greater impact of academia on policy making. Furthermore, students of famous professors do become bureaucrats and thereby transmit certain frames of reference into bureaucratic policy making.

Both of these books indicate that German research and policy making increasingly approximate the American pattern. In both countries policy making is dominated by the bureaucratic frame of mind. Each problem is met as it arises, is treated on its own merits, and is pragmatically resolved by some compromise based on an admixture of objective forces and bureaucratic vested interests. In both countries internal bargaining predominates over substance and long-term planning. In both countries leadership groups show competence in dealing with technical issues. But that is not sufficient. Both the Federal Republic and the United States need creative policy makers who are constrained neither by routine nor by dogma.

CHARLES R. FOSTER

Committee on Atlantic Studies

The Widening Gulf: Asian Nationalism and American Policy. By Selig S. Harrison. (New York: Free Press, 1978. Pp. xi + 468. $15.95.)

In this comprehensive and well-written volume Selig S. Harrison presents the fruits of his extensive Asian experience and his long reflections on the changing scene in Asia and the Asian policies of the United States. His observations are particularly disturbing—and thereby hangs his tale.

This volume is truly impressive in scope and in prescription. It includes a not wholly successful effort to explain Asian nationalism, a multifaceted phenomenon that virtually defies description; a lengthy analysis of nationalism and communism in Asia, prompted, he suggests, by "the special American preoccupation with communism in Asia" (p. ix); a very critical interpretation of American policies in Vietnam and Korea; a critical commentary on American military and economic presence and policies in Asia; many perceptive observations on Asian attitudes toward the United States; and a brief discussion of regionalism in Asia, which he interprets as "nationalism writ large."

In a sense Harrison has written at least two books, one on Asian nationalism, the other on U.S. policies in Asia and Asian reactions to these policies and to the United States generally. These two broad subjects are linked in his mind because he is convinced that U.S. failures in Asia—which he regards as failures of understanding as well as of policy—are due in large part to American insensitivity to Asian nationalism.

Harrison sees "the essential character of nationalism in Asia as a broad response to Western dominance rather than as a reaction to the colonial experience narrowly viewed. . . . Nationalism in Asia is something much more than the assertion of linguistic, cultural, or ethnic identity as such. It is a search for foci of identity and self-respect that can temper the inferiority feelings generated by white, Western dominance. . . . Nationalism becomes a comprehensible concept in the Asian context only when viewed as a multidimensional search for the most appropriate and workable vehicles for the Asian response to Western dominance" (pp. 7, 421). This is a comprehensive and, one might add, a charitable interpretation. It tends to obscure the darker side of the phenomenon, including the use of nationalist appeals to cloak personal aggrandizement, cruelty, and skulduggery, to divert Asian minds from local grievances and failures of leadership, performance and will, to use real or alleged foreign machinations as an excuse for authoritarian policies at home, and to blame others for national failures. It also tends to obscure the distinctive characteristics of nationalism in each Asian country. Harrison is, however, well aware of the idiosyncratic as well as the common characteristics of Asian nationalism. In fact, one of his central theses is that "it is the failure to treat each Asian nationalism separately on its own terms that has been at the bottom of most major errors of American policy" (p. 34).

His main criticism of U.S. policy in Asia is that the United States has been insensitive to Asian nationalism and has not even tried to understand it. He charges that "while thinking of itself as the defender of nationalism, the United States has in actuality misunderstood what nationalism is in the Asian context and has more often than not been its enemy" (p. 36). This theme runs throughout the book. It is the theme of several case studies, including a repeat of "The Pakistan Alliance: Case History of a Mistake" which first appeared in *The New Republic* in 1959, and an examination of U.S. policy in Korea.

Harrison was one of the few American journalists to be permitted to visit North Korea (in 1972). He has been particularly charitable toward Kim Il-sung and the North Korean regime, and particularly critical of South Korea and of America's Korean policy. "From the start," he believes, "American policies have consciously or otherwise helped to divide the country and then to reinforce the division" (p. 237). "Disengagement," he is convinced, "could open the way for eventual reconciliation with Korean nationalism, . . . while continuance of a one-sided commitment will only prolong the agony, adding to a legacy of bitterness that will be difficult to eradicate under the best of circumstances" (p. 244). These views are obviously at variance with those of most American specialists on Korea, and will find more favor in North than in South Korea.

Two chapters each are devoted to the American military and economic presence in Asia. Both are highly critical. The prescription for future U.S. military policy is "detachment." "Detachment would be the most valuable contribution that the United States could make to the evolution of an intra-Asian balance reinforced by a structure of arms control agreements. The capacity of the United States to affect the indigenous momentum of Asian events is relatively limited, . . . but its capacity for distortion through artificial power inputs is infinite" (p. 387). Economically, the United States is "regarded as a latter-day imperialist . . . opposed to independent Asian industrial growth" (pp. 315, 317) and to public enterprise. American private enterprise is pictured as being even more insensitive to Asian nationalism and sensitivities than the U.S. government.

This comprehensive survey adds up to a damning indictment of U.S. policy in Asia. It can easily be criticized for its many sweeping generalizations. It is as uncritical of Asian nationalism as it is critical of America's Asian policies. One may question whether the gulf between Asian nationalism and American policy is indeed widening, as Harrison contends. There are encouraging evidences that the trend may in fact be in the other direction, as both Asian nationalism and U.S. policy become more responsible and realistic. But Harrison has rendered a service by calling attention to various critical Asian attitudes toward the United States that have affected, and will continue to affect, U.S.-Asian relations. His book should interest anyone who is concerned with the changing Asian scene and with U.S. policy toward Asia. It should be must reading for all Americans who will be involved in future decisions regarding Asia.

NORMAN D. PALMER

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Command Decision and the Presidency: A Study of National Security Policy and Organization. By R. Gordon Hoxie. (New York: Reader's Digest Press, distributed by Thomas Y. Crowell Co., 1977. Pp. vi + 505. $15.00.)

The value of this book lies in the sections which deal with its subtitle—national security policy and organization. It is much less worthwhile where the subject of presidential "command decision" is concerned. This unevenness stems from the author's failure to decide whether he wanted to do a serious study of the American defense establishment's capacity to meet the demands of world leadership or write a polemical essay defending an "imperial Presidency" to deal with a world which he believes had hardly changed during the past quarter century.

Gordon Hoxie has dedicated his book to two presidents, Truman and Eisenhower, as "the builders of our national security policy." But while he admires Truman, Eisenhower emerges as the model president, a demigod worthy of resurrection and adulation. After our experience with Johnson, Nixon, and Ford, Ike does indeed look better. But like other Eisenhower revisionists, Hoxie somehow manages to exonerate the general from responsibility for the views or actions of his own subordinates, as when he says that Mr. Eisenhower was from the beginning of his tenure determined "to wage peace despite war hawks in the Congress, despite the China lobby, despite the 'brinksmanship' of Secretary of State Dulles, and despite the Chairman of his Joint Chiefs of Staff, Radford, who was especially spoiling for a showdown with Communist China" (p. 204).

Although his approach to both Eisenhower and Truman is uncritical, Hoxie is on firm if familiar ground in describing the post-World War II reorientation of American foreign policy. This period was a watershed in our history, and both the policies and the institutions which came out of it have charted America's course in the world ever since. The doctrine of containing communism has gone through various stages, but it still remains the cardinal principle of American foreign policy; and in the first (and best) part of his book Hoxie traces the development of that policy from the initiation of the Marshall Plan through the establishment of the North Atlantic Treaty Organization to the global system of alliances developed by Eisenhower and Dulles. Within this framework, he also shows how the government streamlined itself to meet the challenge of world leadership and to implement its new policies. His study of the National Security Act of 1947 in particular is solid and informative, tracing the difficult fight for armed forces unification. The act was a landmark of the post-war period (although its purposes were not fully realized for a decade) and Hoxie tells the story well, emphasizing the vicious interservice rivalries, the mixture of policy and politics, the roles of Congress and the president in achieving the compromises necessary for enactment of the legislation, and—in James Forrestal's suicide—the human toll exacted by the frustrations and disappointments of the battle.

Hoxie's troubles begin when he leaves the Truman-Eisenhower era and moves into the more complex territory beyond the cold war. Except for the Cuban missile crisis, the second part of his book eschews detailed description of policy development and proceeds, on a meandering course, toward the thesis that as a result of Vietnam and Watergate "we have tied the hands of the President and Commander-in-Chief in the area of foreign policy" (p. 399). Leaving aside for a moment the issue of whether Hoxie's view is valid (and only noting that the materials are badly organized, unsystematically presented, and glaringly repetitious), his failure to analyze those specific uses of power that led to attacks on the "imperial presidency" is a fatal substantive flaw. In the end what we get is not serious discussion of an important problem—whether the power of the contemporary presidency is too great or too limited—but only the sounding of an emotional alarum. This results from the fact that Hoxie is an old cold-warrior whose perceptions of the national interest are basically chauvinistic and whose approach to national security is primarily militaristic. He hankers after the Truman-Eisenhower years when decision making in foreign affairs was, or seemed to be, simpler, when the world was more neatly divided between the good guys and the bad guys. In the cold war atmosphere of the 1950s a new American president would not have disappointed Mr. Hoxie by making such "wrong decisions" as cancellation of the B-1 bomber program (p. 387).

Hoxie is correct in attributing the change to Vietnam and Watergate, and he properly records the various attempts by Congress over the past five years to circumscribe presidential power in such areas as covert domestic and foreign operations, impoundment of funds, executive privilege, and independent warmaking. But whether in the wake of this outburst of congressional assertiveness it is possible to say that "no other democratic society has so emasculated the role of the Chief of State in foreign policy in the decade of the 1970s as has the United States" is questionable (p. 380). Certainly Mr. Carter's Panama Canal treaties and his interventions in the Middle East and Black Africa do not support such a conclusion.

There is no evidence—either in this book or in current affairs—to indicate that the presidency has been rendered incapable of dealing with today's difficult and dangerous world. And there is no discernible reason for a panicky rush to reconstitute the office as it operated under Johnson and Nixon. In this book Hoxie may have contributed to our understanding of why and how the presidency came to have virtually unlimited power in the realm of national security affairs, but he does not convince us that such an institution is essential to our survival. We can only hope that his kind of nostalgia for the past will not shape the future.

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The World Council of Churches in International Affairs. By Darril Hudson. (Leighton Buzzard, Beds., U.K.: The Faith Press for The Royal Institute of International Affairs, 1977. Pp. 336. £5.40, paper.)

It was in the name of "science" that realism supposedly triumphed during the 1950s as a paradigm for global political analysis. The present work apparently recognizes that "placing the World Council and NGOs into the traditional concept of international relations—if that concept sees international relations as being solely a struggle for power—does not account for their continued activity in international affairs, nor for the institutionalization of their relationships with intergovernmental organizations" (p. 21). Yet this book represents a resurrection of an even older and more unsatisfactory tradition, a "moralistic" blend of quasi-journalistic description and value judgment. In terms of purportedly explanatory concepts, Darril Hudson apparently has little trouble relying upon realistic bugaboos of states' "vital interest" (p. 290) and "national interest," to which are added the equally problematical idea of "international interest" (pp. 262, 277).

This book is about what the ecumenical movement has done in recent international history. It is organized into five chapters, plus a brief introduction and an epilogue which summarizes the 1975 World Council of Churches meetings in Nairobi. The first chapter traces the evolution of the WCC. It is followed by chapters detailing WCC activities regarding race (emphasizing South Africa—the "White Devils" of the chapter title?), development (both economic and human), and disarmament. We supposedly may compare WCC involvement in these issue-areas, although no attention is paid to distinguishing them in a theoretically meaningful fashion.

The final chapter attempts to argue that "the World Council has acted in the same way as interest groups at the national level act when they wish to influence public policy" (p. 261). "A Communications Model of the WCC in International Decision-Making" (treated on pp. 278–82) is, however, little more than a drawing. While not altogether inaccurate as illustration, its connections with the previous materials are never really evaluated. "Communications" as used in this work seem to mean nothing more than having some kind of contact, and the "model" provides no valid way for understanding a situation of "Manifold Complexity," the subhead under which the model is encountered.

This might have been a good book. A nonstate actor like the WCC, expending upwards of $16 million in 1975 (p. 276), is not insignificant to global outcomes, although this work does not actually demonstrate the fact. Analysis, or even speculation, about how WCC successes stack up against those of various religious groups or other kinds of nonstate actors would have been welcome. As it is, we are left with the author's opinion regarding whether different WCC activities have been ethically right or wrong, and a sort of implicit envy for the Vatican "being treated as a government" (p. 201; also pp. 182–84, 271, and passim). Empirical analysis aside, this work could have found the lacking theme by following through on almost hidden suggestions that the "Anglo-Saxonism" of the WCC had led to a kind of "cultural imperialism" (pp. 35, 36, 44, 156, 280, and passim). Sadly, the author is less concerned with pursuing interesting points he at least covertly raises, some of which are admittedly arcane, than with continuing his narrative. An example of this tendency is a persistent and ill-defined use of "witness" (as both noun and verb) as virtually a social scientific concept. Indeed, if the reader, like me, is not a regular churchgoer or has not recently attended Sunday School, many of the author's comments simply pass one by, although a list of abbreviations and chronology of WCC meetings does prove somewhat helpful.

Nonetheless, Hudson has done a kind of yeoman's work by sparing other scholars the task of sifting through various WCC publications. Unfortunately, this work's unquestioning acceptance without definition or explicit demonstration of pluralism automatically transcribed to world politics is at best naive and at least unsatisfactory. Eight of the twelve WCC meetings listed since 1968 have been, like this book, apparently, lacking in a "main theme" (pp. 295–96). No one should deny the WCC its noble projects, but it would seem safe to predict that the future of this book will be a far from undeserved status on obscure library shelves where it can serve undergraduate term papers, "relevant" sermons, and non-Scriptural weeks at Bible classes. Ultimately, it is based more on "faith" than analysis.

DONALD E. LAMPERT

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The Domestic Context of American Foreign Policy. By Barry B. Hughes. (San Francisco: W. H. Freeman, 1978. Pp. xii + 240. $14.00, cloth; $7.00, paper.)

Relying on the "massive volume of survey data" and other information now available, Barry Hughes' book is a well-organized and quite readable effort to extend and update Gabriel Almond's classic 1950 study, *The American People and Foreign Policy*. Hughes briefly reflects on various decision-making models and tries to see how well the data fit. Drawing on the extensive available literature, he seeks to determine how the American people feel about foreign policy, how American decision makers feel about it, and how, if at all, the former affect the latter—whether directly, or through parties, interest groups, and/or the press.

Among the most interesting parts of the book is an effort near the end to divide issues into various categories and to assess the most typical forms of influence in each case. Rather than assuming that all foreign policy issues are identical in their influence structure, Hughes roughly, but interestingly, dichotomizes them by the length of time available for decision and by the degree to which they reflect economic or security considerations. The conclusions tend to be declarative and the evidence anecdotal, but the observations that emerge from this analysis are often provocative, if sometimes a bit slippery.

The bulk of the book is taken up with an evaluation of the available evidence concerning public and group influences on foreign policy decisions. It's a rather vast field and in an effort to cover it all, Hughes often has to resort to a once-over-lightly treatment. For example, one chapter presents quite fascinating longitudinal data on congressional voting patterns on various foreign policy issues between 1948 and 1974. Hughes assesses these data thoughtfully, but hints along the way (e.g., p. 134) suggest that the general character of the data (the attitude on a given issue is apparently assessed from a single vote in each Congress) hides peculiarities that, directly considered, might alter some of the conclusions. More intensive analysis might be more convincing, although it would, of course, shift the focus of the book.

The quality of the public opinion data analysis varies considerably.

Hughes often has a good flair for sensible subdivisions—for example, in his distinction between military and nonmilitary international policies. Following Almond's lead, he is usually quite careful about separating the various foreign policy publics according to their attentiveness and awareness of the issue at hand. And he generally respects the fact that the data, no matter how massive, may still be quite inadequate: evidence on one point, he observes, is "extensive but fragmentary"; surveys concerning another issue are found to be "infrequent, irregular, and generally not comparable."

Nevertheless, there are quite a few instances when the analysis could have been done more carefully. Hughes concludes that the "relative interest by the public in domestic and foreign issues remains quite stable over time," basing this on only three data points and ignoring the extensive fluctuations in other trend lines designed to measure this phenomenon. And a chart on p. 39, later used to make a substantive point about the public's influence on policy, is simply misdrawn.

Although Hughes seems to be aware that changes in question wording can alter the public's putative "attitude" toward an issue enormously, most of this recognition is tucked into footnotes. And he is content to argue, for example, that a "majority" of the people have consistently supported foreign aid or that a "majority of those with opinions" came to oppose the Vietnam War in 1967–1968. Differently worded questions could easily reverse these majorities, or make them bigger.

Perhaps inevitably in a book concentrating on the domestic context of foreign policy, there is sometimes an underappreciation of the impact of foreign events themselves on that context. Thus the effectiveness of the China lobby in influencing the country's position on the admission of Communist China to the United Nations is found to be "remarkable"; the effect of the experience of the Korean War on that position is not assessed.

In general, then, Hughes' book brings together considerable information on his chosen subject, handles much of it with restraint and care, and sorts out conclusions that bear examination if not uncritical acceptance.

JOHN E. MUELLER

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The Soviet Union and International Oil Politics. By Arthur Jay Klinghoffer. (New York: Columbia University Press, 1977. Pp. ix + 389. $16.50.)

Arthur Jay Klinghoffer has filled a notable gap in the literature with this comprehensive and thoroughly competent account of Soviet involvement in the highly complex game of international oil trading. The book is based on extensive research and covers a wide range of topics, from general consideration of the "political economy of Soviet foreign trade," "domestic foundations of Soviet energy policies" and "the strategy and tactics of Soviet oil politics" to specific accounts of Soviet relations with all the major blocs of nations (on a country-by-country basis) in matters relating to the international trade of energy resources. Although some redundancy is unavoidable, the organization of the book is admirable, and it is easily usable as a reference source.

The picture that emerges from these pages is broadly familiar to those in the business of Kremlin-watching. From modest and hesitant beginnings, the Soviet Union has become increasingly sophisticated in the use of the "oil weapon" in the pursuit of its international policy goals. Certainly the very close inter-

twining of political objectives and economic terms-of-trade in Soviet policy is demonstrated repeatedly in the cases reviewed in this volume. There is also ample evidence reported here that the Soviet Union is usually alert to opportunities to earn a fast petro-buck through various trilateral "switching" arrangements (using foreign purchased oil to satisfy its export commitments), exploitation of oil embargoes and rising world market prices, and attempts to attract foreign investment capital and advanced technology through oil barter deals.

One of the hazards of writing a comprehensive survey of such a rapidly changing subject is that it may already need revision on the day of its appearance, especially if there is a three-year hiatus between the completion of research (apparently in this case sometime in 1974) and the date of publication. Although the major portions of the book are by no means obsolete, there are sections, particularly those dealing with the Horn of Africa, the Middle East, and Soviet relations with Japan and the United States, which would benefit from an up-date. Klinghoffer's assessment of Soviet oil reserves, made before the 1977 CIA downward estimate, is that the Soviet Union possesses "ample fuel reserves, but it is still faced with an oil problem" (p. 280) in the future, because production will probably not keep pace with increased domestic and bloc demand. On the basis of this estimate of Soviet self-sufficiency in oil production and reserves, Klinghoffer depicts Soviet oil trading as an instrument of foreign policy, rather than a response to energy shortages. Thus having a relatively free hand in its oil trading policy, the Soviet Union is described as pursuing two policy objectives: the acquisition of hard currency reserves and advanced technology, and the acquisition of influence and strategic advantages.

The interplay of these policy goals is nowhere better described than in Klinghoffer's fascinating account of the complex negotiations over the years for Japanese and/or American collaboration in the construction of proposed oil and gas pipelines in Siberia, close enough to the border to stir up Chinese apprehensions. The complicated interaction of economic, military and political objectives of the four countries involved is extremely well presented. Also admirable is the exposition of Soviet policy toward the Arab oil-producing states, particularly during the period of the OPEC oil embargo.

Throughout the discussion of the multitude of Soviet oil dealings, Klinghoffer presents Soviet policy as a reasonably harmonious blend of economic, strategic and political considerations. *The possibility of conflict among these* objectives, or among the bureaucracies which support them, is not given much attention; Klinghoffer rather glibly asserts that the Soviet government "can easily harmonize foreign policy with state economic interests as a result of state ownership . . . and a nationalized system of foreign trade" (p. 9). Thus Soviet oil policy is seen, overall, as the rather well-coordinated and effective product of a well-integrated state bureaucracy. Although the evidence sometimes indicates apparent contradictions in Soviet policy, Klinghoffer explains these as cases where the Soviet policy makers are "hedging bets" or "keeping a foot planted in the enemy camp." He writes: "Occasionally, there appear to be discrepancies in Soviet oil policy but they are generally intentional and are not dysfunctional" (p. 296).

While such an approach does no great harm to the main purpose of the book, a descriptive survey of the foreign oil policies of the Soviet Union, it does create an illusory image of Soviet decision making and limits the potential usefulness of the book as a case study of Soviet foreign policy making. Although Klinghoffer mentions *en passant* that "policy and organizational dilemmas" exist and that "the Soviet system is not completely monolithic, as indicated by occasional bureaucratic differences" (p. 290), he does not pursue these areas of conflict, but rather contrasts the Soviet system of bureaucratic hierarchy with the relatively pluralistic American system, where major oil companies can and do have "foreign policies" which manifestly differ from that of the U.S. government. Of course, there is a big difference here (need we be reminded?), but this should not serve to obscure the realities of Soviet policy making, where conflict often lurks just beneath the deceptive surface calm.

Certainly this is true of domestic oil policy, where considerable debate has been publicly aired on questions of Siberian oil development. Today the Soviet Union faces a problem of whether (or how rapidly) to develop increasingly expensive internal reserves of oil and gas, or become increasingly dependent on other energy sources. Even if the CIA has underestimated Soviet reserves, the time will surely come when the USSR will not be able to produce sufficient quantitities to satisfy its internal needs and external commitments. At that point, or somewhat before, the USSR may enter the world petroleum market as a large-scale buyer of a scarce resource, and the pattern of its past behavior, so well described by Klinghoffer, will undoubtedly change. In the past, he argues, the Soviet leaders "have been careful not to use oil so politically that other states are unwilling to trust them as a trading partner. They have used oil politics judiciously. . ." (p. 296). One wonders whether such commendable restraint, based on past and present self-sufficiency in fossil fuels, will continue on that future day when the Soviet leadership is confronted for the first time with an oil shortage.

JEROME M. GILISON

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Post-Mao China and U.S.-China Trade. Edited by Shao-chuan Leng. (Charlottesville: University Press of Virginia, 1977. Pp. xi + 156. $9.95.)

A collection of seven essays presented in 1976, this volume purports, in broad terms, to examine the problems of U.S.-China trade in the post-Mao era. In specific terms, the symposium was meant to study at least nine questions concerning China's foreign trade (p. vii). Of these, three questions were directed toward the role of trade in U.S.-China relations, the prospects and problems of U.S.-China trade, and the type of trade with China that is "compatible" with U.S. national interests; another three questions toward the political aspect of China's foreign trade, both internally and externally, with special reference to the impact of Chinese leadership succession on China's foreign and trade policies, the relationship of trade to politics, and the relationship of China trade to the international balance of power; two questions toward the past performance of China's foreign trade, the types of commodities that are most likely to be exported and imported by China in the future, as well as the types of services that may be in demand; and the remaining one question toward the techniques of negotiation with the Chinese.

If readers anticipate the volume to be a guide to doing business with China or hope to learn something from it about the United States' policy and practice of trading with China, they are likely to be disappointed: except for Robert F. Dernberger's economic appraisal of U.S.-China trade on the basis of the empirical data he has gathered (pp. 79–89) and Nai-Ruenn Chen's general discussion of China's foreign trade prospects in the context of the Chinese national economy (pp. 40–44), the rest of the volume is virtually silent on its major theme–trade between the United States and China.

Individually, however, Robert A. Scalapino makes some meaningful political analysis of the recent power struggle in China and predicts that barring another cultural revolution, "the prospect is for economic expansion in the balance of the 1970's" (p. 48). Citing official Chinese publications on the opposition of "the gang of four" to China's foreign trade policy, Tao-tai Hsia raises an interesting question as to whether such denunciations stemmed from the need of the gang's opponents to exaggerate the radical attitude of the four for polemical effect, or whether the radicals in fact were forced into an essentially isolationist position by their need to attack every initiative of the moderates (p. 97). Hsia himself, however, does not answer the question.

Eugene A. Theroux offers some insights into the practice of China's foreign trade, emphasizing the fact that on the basis of his personal experience the Chinese mean what they say and those Americans who have ignored Chinese statements did so at their own "peril" (p. 56). In other aspects of his discussion, however, Theroux does not provide necessary documentation (e.g., pp. 60–62) and his list of Chinese trade principles and institutions, previously published in a compendium sponsored by the Joint Economic Committee of the Congress, is incomplete.

The chapter on "China's Oil Policy," prepared by Jerome Alan Cohen and Choon-ho Park, is well documented and its discussion of China's oil exports to Japan is particularly revealing. However, like many other essays in the volume, Cohen and Park do not touch on what I consider a central question of the symposium, namely, the implications of China's oil policy for trade with the United States and the possible reaction of the international oil consortiums to the growing imports of Chinese crude by the Japanese oil industry in which the consortiums have a very large stake.

Ralph N. Clough in the final chapter discusses the "inseparability" of politics from trade in Chinese relations with Japan, West Germany, Canada, and Australia as well as the political implications of Chinese aid programs. In all these instances, he only offers a cursory review of past events on a selective basis without meaningful documentation. In fact, he either ignores or overlooks all existing academic works on the same subjects except for the volume edited by Arthur A. Stahnke (p. 153). The result is that he not only misses many important facts but also oversimplifies his recitation of those complex international issues.

International trade is a two-way traffic. The present volume, however, is one-sided: it has left out the United States' role in this bilateral trade. And even in those selected areas of study on Chinese politics, economy, trade institutions, and past performances, the discussions are not always in accord with the questions mentioned above. For example, there is virtually no discussion of the role of trade in future U.S.-China relations. Admittedly, not all discussions at the symposium were included in this volume. But it is evident that the organization of the volume is not quite consistent with its major purpose.

Gene T. Hsiao

Southern Illinois University

Africa's International Relations: The Diplomacy of Dependency and Change. By Ali A. Mazrui. (Boulder, Colo.: Westview Press, 1977. Pp. x + 310. $23.75, cloth; $15.00, paper.)

Since David McKay wrote a pioneering work on the international relations of Africa in 1963 (*Africa in World Politics*), only a handful of books, including anthologies, on the subject have been published. Ali Mazrui, one of the most prolific African political scientists, has made another contribution to the literature. This rather thin volume is probably the most comprehensive work on the subject: its topics range from African political ideologies and the relations between Africa and the four other continents, to contemporary global issues such as race, population, disarmament, and ecology. It looks like a compendium of all Mazrui's previous works. A cursory look at the references after each chapter shows that, to some extent, this is the case. However, this does not mean that no new theses and ideas are presented in this book. What Mazrui does here is to review and assess contemporary African issues in the light of the past, and predict future trends in terms of the hypotheses and analytic frameworks he has developed in his old works.

The book consists of three interrelated parts. The first part formulates two broad analytic frameworks. First, it characterizes the contemporary international system as a caste system rather than a class system, in which the rich countries are compared to the Brahmins and the poor countries to the untouchables. (In my opinion, however, the North-South conflict can be characterized as a conflict of classes rather than of castes.) Secondly, Mazrui discusses how African nationalists used Western liberalism for national self-determination before independence and how they are using "Western" Marxism for the anti-imperialist struggle after independence. These two borrowed ideologies, he concludes, are to serve the cause of African nationalism. One serious flaw throughout this argument is that Mazrui confuses liberalism with democracy.

In the second part Mazrui discusses the relations between Africa and the Black Diaspora, Africa and Asia, Africa and the Middle East, Africa and Western Europe, Africa and the U.S., Africa and the USSR, and finally Africa and the United Nations. In examining these seven pillars of relationships, the author does not apply the two grand analytic frameworks developed in part 1; instead, he surveys these relationships in a very broad stroke from a historical perspective, and philosophically speculates about future trends.

In dealing with each of the seven relationships, Mazrui makes some provocative arguments which, as the flyer suggests, will stimulate dissent, if not thought. He proposes a black commonwealth of nations similar to the British Commonwealth. However, this commonwealth would not consist of independent states but rather of all black peoples including Asian aborigines. Comparing Western civilization and African civilization, Mazrui touches on one of the most sensitive questions of today: is the black race innately inferior to the white race? He argues that blacks are "inferior" to whites only in technological fields and goes on to offer five explanations which are all based on circumstantial, not biological, factors. These explanations, however, are not entirely satisfactory, partly because some of them are tautological (e.g., why did Africans not invent the written word and the wheel?).

In his discussion on the relations between Africa and the Middle East, Mazrui contends that one of the reasons why black Africans support the Palestinian cause in the Arab-Israeli conflict is that they see similarities between apartheid and Zionism. Thus, he provides a connecting link between Zionism and apartheid.

Commenting on the U.S. policy toward southern Africa, Mazrui opposes U.S. economic disengagement from South Africa as a means of coercing South Africa to abandon its apartheid policy. This position has been propagated by some multinationals in South Africa. Mazrui's argument reveals his commitment to conventional liberalism, which he proudly admits (p. 204). Some American liberals predicted years ago that the Soviet Union would inevitably evolve into a liberal society as it matured industrially. However, the question is far from resolved: is the present Soviet system an eroded form of totalitarianism or a more sophisticated form of totalitarianism? In which direction would South Africa move?

Part 3 is devoted to world issues as related to Africa. Mazrui's proposals for fighting racism are well reasoned and should be considered seriously by policy makers. However, his views on the demilitarization of Africa and population control in Africa seem to be based on a misinterpretation of the Western positions. He gives the impression that Western countries advocate African disarmament and population control in order to keep Africa weak. I am not sure whether these Western positions on Africa are entirely independent of the West's commitment to disarmament and population control in the West itself.

Mazrui's discussion of the African view of ecology is rather philosophical. He argues the Western position is derived from ecological curiosity, whereas the Africans' is derived from ecological concern.

From a methodological point of view, Mazrui demonstrates a rare intellectual brilliance. He frequently uses dichotomous schemes of classification which are genuinely original and sometimes surprisingly witty. These include his classifications of dependency into submissive and aggressive dependency, of colonialism into a structure of domination and a structure of damnation, and of Pan-Africanism as a movement of liberation and integration. He often uses dichotomy for dialectical reasoning. His futuristic arguments are often predicated on a synthesis of two opposing theses. Sometimes his mastery of the English language does some damage to his otherwise highly insightful discourses, as too sharp a kitchen knife cuts too deeply.

In conclusion, *Africa's International Relations* is a milestone, if not a magnum opus, in Mazrui's colorful academic career. It is suitable as a major text book for undergraduate courses in African international relations and as a supplementary reading in international relations courses.

SANG-SEEK PARK

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Deterrence: A Conceptual Analysis. By Patrick A. Morgan. (Beverly Hills, Calif.: Sage, 1977. Pp. 216. $11.00, cloth; $6.00, paper.)

Bargaining with the threat of force long has been a phenomenon of international politics. For centuries, statesmen and generals have attempted to use their potential physical power as a means of manipulating others. The traditional practice of coercion and deterrence in the past, however, seldom was accompanied with any explicit theory or conceptual framework. It was simply recognized that opponents frequently modified behavior in order to avoid the threatened pain that followed military destruction. This rather simplistic bargaining approach changed when sophisticated nuclear weapons and delivery systems made it possible for states to make a distinction in fact as well as in theory between the threat to hurt an enemy and the threat to destroy his forces. Such a development in the context of the cold war rapidly led to an imposing body of theory created primarily by American scholars and dealing primarily with bargaining techniques in nuclear deterrence. Professor Morgan's *Deterrence: A Conceptual Analysis* is written to analyze and criticize this body of theoretical literature, as he states (p. 15), in the "hope that this book will contribute to a better understanding of deterrence in theory and practice, an understanding that will lead us to reconsider our willingness to continue entrusting our fate to our capacities for violence."

Morgan begins with a general discussion of the subject of contemporary deterrence theory, with a particular emphasis upon what he considers to be its faults. He notes wide discrepancies between various theorists when discussing precisely the same issue. These can be found with reference to problems of definition, description, the unspecified nature of adversaries and threats, and the ingredients that might make deterrence a successful strategy. He believes that much of this confusion results from the failure to distinguish between what he terms the common form of "general deterrence" involving opponents who maintain armed forces to regulate their relationship over time, and the rare form of "immediate deterrence" involving opposing states where one is mounting a threat of retaliation in order to prevent an armed attack by the other.

The most perceptive and stimulating analysis occurs when Morgan examines the interrelationship between theories of deterrence and theories of decision making within the context of political forces. He accuses deterrence theorists of possessing such a strong bias for a model of rationality among human actors that they completely ignore many aspects of policy formulation. "Different conceptions of how governments make decisions, pitched at different levels of analysis," he writes (p. 49), "produce different images of deterrence, how it works, and what the chances are of success." To substantiate his claim, Morgan provides a marvelous systematic evaluation of the alternatives and of the implications of working at one level rather than the other. This includes a treatment of the individual level of analysis (personal characteristics of decision makers, perceptions, rationality, and cognitive theory), the group level (organizational theory, process models, and bureaucratic politics), and the national actor level (sovereign states considered as aggregated, rational units). These various abstract and apolitical approaches are then examined in the context of the practical political considerations that face all decision makers at all levels of analysis. Finding errors, inconsistencies, and ambiguities throughout the theoretical literature, Morgan believes that our present concepts of deterrence are much too simple and dangerous. He concludes—as do Alexander L. George and Richard Smoke in their recent book—that one therefore must call into question the wisdom and utility of relying so heavily upon deterrence to sustain the nation's security and its place in the world.

Deterrence: A Conceptual Analysis is an important contribution to the growing debate over the role and conduct of deterrence in international politics. Although readers may find portions somewhat overdrawn and highly personal, they will discover that the book as a whole is extremely perceptive, imaginative, bold, and provocative. This book will shake the confidence of those comfortable with present deterrence policy, will arouse controversy among strategists of bargaining, and will encourage theorists to be more sensitive to the complexities of the human condition.

PAUL GORDON LAUREN

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The Philosophy of International Relations: A Study in the History of Thought. By F. Parkinson. (Beverly Hills, Calif.: Sage Publications, 1977. Pp. 245. $14.00, cloth; $6.95, paper.)

This book's subtitle is more indicative of its contents than the main title. It is the most up-to-date study of the history of thought on international relations, although not, as the author claims, the only comprehensive one: Frank M. Russell's "Theories of International Relations" (1936) is another one, and, with chapters on ancient Chinese and Indian thought, even more comprehensive.

Parkinson's survey is rather uneven. There is much material not easily found elsewhere, and some of the more familiar material is restated interestingly. But there also are serious omissions and misinterpretations.

The book starts with a sweeping chapter (16 pages!) on ancient Greece, Rome, and the Middle Ages. Some trends of thought (such as the Stoics') are covered adequately, but there is nothing on classical Greek thought on intercity relations, as revealed by Thucydides, and the less said about what Parkinson says about Plato and Aristotle, the better. In chapters on the sixteenth and seventeenth centuries there is interesting analysis of the Spanish theologians (Suarez, etc.), also of Machiavelli, but there is little on thought connected with the core phenomenon of the age, the rise of the "sovereign" state; Bodin is not mentioned. And the most significant school of thought on the early modern state system, that of *raison d'état,* is slighted, as is its counter-tvpe, that of the utopians (Abbé de St. Pierre, etc.). Instead, balance-of-power ideas are interpreted, unconvincingly, into the latters' schemes. Otherwise, there is interesting material on balance-of-power theories in general.

Since after a thorough chapter on the Enlightenment philosophy, Parkinson proceeds less chronologically, I shall mention only a few fortes and weaknesses of his subsequent analyses. What he has to say on Marxism, from Marx to Stalin, and also on other socialist and "revolutionist" thought, belongs to the best in the book; it is always critical but objective. One appreciates particularly an analysis of recent dependency theories. Nationalism is discussed in its earlier, Herderian version and its impact on multi-ethnic units (Austrian empire), but there is nothing on the expansionist type or on theories of imperialism of the colonialist or Nazi-fascist versions. A chapter on federalist theories neglects the Federalist Papers, and while there is something on the ideas that preceded the founding of the League of Nations, there is nothing comparable on the UN system or international organization in general, nor on thought connected with the rise of the Third World (on "wars of liberation," for instance, or on recent claims for a new economic world order). There also is nothing on Europeanism.

In a lengthy final chapter, the author gropes for synthesis. Perhaps despairing of teasing some unity out of the jumble of recent "scientific" theorizing, he singles out a simulation technique, workshops for conflict resolution, as possibly pointing towards developing theory, placing his hope in conductors ("mediaries") trained in intergroup therapy and students undergoing "gentle psychotherapy." As he admits, so far results have been slim.

Like most historians of international relations thought, Parkinson fails to pay sufficient attention to the questions of what functions the different theories fulfill; how they relate to the "realities" of international relations; how they relate to the "pretheories," concepts, ideologies, and other thought-patterns that form the conceptual framework of actors in foreign affairs; their division into prescriptive and descriptive types; and to what *extent* they cover "reality." How *do* theories relate to "realities"? If one undertakes to build up a theory of human relations by applying Kantian epistemology, social structure emerges as the sum-total of humans' impressions of "reality," their perceptions of individual and group relationships and of the international world as part of this social web—in short, their awareness of that (now global) world, their "world views." This *Weltanschauung* enfolds in historical stages and on different levels. On that of actors (statesmen, diplomats, other leaders, and "attentive publics") it appears usually as a jumble of perceptions, misperceptions, non-perception, ideologies underlying "movements," and so forth. On the level of theorists (philosophers, intellectuals, etc.) the world of awareness may be more refined and less "slanted," but its concepts often become absorbed by actors and others as "topoi," slogans, received opinion, misinterpretations of their original meaning (see Wilson and his interpretation of balance of power as crude "power politics"). Bits and scraps of theory, then, become, in Richard Cox's words, "layers of philosophical and doctrinal accretions" that "encrust" our views of political life. These interpenetrations of views and concepts have to be sorted out if one is to assess the history of political thought. Studies like Parkinson's may serve as starting-points for reflecting on how international relations must be analyzed to form up-to-date theory.

JOHN H. HERZ

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From June to October: The Middle East Between 1967 and 1973. Edited by Itamar Rabinovich and Haim Shaked. (New Brunswick, N.J.: Transactions Books, 1978. Pp. xxiii + 419. $19.95.)

This volume contains 22 papers presented at a seminar and colloquium late in 1974 at the Shiloah Center for Middle Eastern and African Studies at Tel Aviv University. Known academic and other specialists, mostly Israeli, analyze the last Arab-Israeli interwar period and the impact of the October War in a variety of areas.

William Quandt introduces external power involvement with a survey of the Arab-Israeli conflict in American policy. Written with a certain detachment, this informative piece reflects the perspective of official Washington, to which the author belongs, in the National Security Council. Oded Eran traces the dilemmas of Soviet involvement, from which it appears that the conventional concern—whether Moscow really desires an Arab-Israeli settlement—almost entirely misses the point of Soviet policy. Rainer Büren charts the steady upgrading of the Arabs in West German policy.

As concerns strategic aspects of the period, the rise of Soviet military power in the region is the focus of Geoffrey Kemp's contribution. While noting the veritable avalanche of advanced arms technology from both East and West in recent years, he sees no real turn yet against Israel in the military balance. Amnon Sela finds that the Arabs did better in the last war, not because they transcended a limited capacity for modern war of movement in depth, but rather because they scaled operations to fit the limits, putting mass, surprise and technological innovation to sharpen a reduced bite. Yair Evron suggests some interesting correlations between the outbreak of Arab-Israeli war and the arms race, ideology, and diplomatic activity.

The economic section focuses on Egypt's plight and Saudi riches. Eliezer Sheffer believes the latter (and Western investment) can turn Egypt around, while Eliyahu Kanovsky argues that infusion of foreign wealth means no real economic development in a society suffering from structural ills (regarding skilled personnel, mainly) like Egypt's. Benjamin Shwadran finds the dynamic of Saudi oil price policy to be essentially economic, tracing its success to the disunity of petroleum-importing states.

Of greatest interest to Middle East students here are the 10 papers by Israeli specialists on the Arab world, mostly brief and detailed monographs. Inter-Arab relations in the interwar period, explains Daniel Dishon, were marked by the fading of institutional unity, ideological dichotomy, and Egyptian leadership, though the centrality of Egypt persists. Gabriel Ben Dor suggests that this very decline in striving for unity allowed the PLO to play the role of supreme Arab value. Varda Ben Zvi discusses the Federation of Arab Republics. Shimon Shamir compares Nasser's and Sadat's approaches to the crisis. Itamar Rabinovich writes on Syria's Ba'ath regime. Elie Rekhess and Asher Susser discuss the politics of the West Bank Arabs. Uriel Dann has pieces of Jordan and Iraq. Haim Shaked traces developments in the Arabian Peninsula (with Tamar Yegness) and Sudan and Libya (with Esther Webman).

The concluding section deals with Iran and the Gulf. Rouhollah Ramazani, Aryeh Shmuelevitz, and Moderchai Abir present different perspectives on the relation of the Middle East core area to the Gulf, the Arab-Iranian interaction, oil policy, and superpower involvement. For those not familiar with Gulf affairs there is bound to be fascination with the intricate complexity and overly sophisticated character of Iranian policies for Israel, the Arabs, and the foreign powers.

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Conflict in Perspective. Understanding Conflict and War, Vol. 3. By R. J. Rummel. (Beverly Hills, Calif.: Sage Publications, 1977. Pp. 200. $12.95.)

Some theory written by political scientists is little more than subtle, critical, and sometimes pontifical analysis of the bold and original thought of others. Some theory written by political scientists is not such emendation of earlier writers but is little more than a translation into current cant of ideas that originated decades or centuries ago. We have a lot of warmed-over Madison and sentimental Marxism to endure. Most theory written by political scientists has lacked freshness because it goes no deeper into psychology, in the twentieth century, than Hobbes, Locke, or Rousseau went in the seventeenth and eighteenth. In the twentieth century, psychology in many of its subdisciplines has become a full-grown empirically scientific body of knowledge, and much of it is relevant to political theory.

It is no longer excusable to analyze political behavior, which involves human beings, on systems-theory bases which often regard the human psyche as an intrusion into systematic systemic models and into clear-cut data on the externals of behavior. This is no longer excusable in the study of orderly behavior; it is quite impossible in the study of either violent or nonviolent conflict behavior.

Conflict in Perspective frankly uses and to a refreshing degree joins Rummel's knowledge of psychology to a consideration of the basic question of conflict and its relationship to political change. The book is quite abstract and even a bit formalistic, but does not succumb to the latter disease. And it does not turn away into rhetoric from a direct consideration of what conflict, aggression, and justice (that ever-elusive term) mean. The book does not achieve the fiery relevance of Georges Sorel's *Reflections on Violence* (first published in 1906, just eight years before the First World War); of the psychologically intuitive depths of Georg Simmel's *Conflict* (1908); or the anguished, surgical soul-probing of Frantz Fanon's *The Wretched of the Earth* (published a half-century later, in 1961). But the book goes farther than any of these in clarifying the relationship among conflict, aggression, and justice.

Like Sorel, Simmel, and Fanon, Rummel emphasizes that conflict is a necessary and inevitable step in the process of resolving differences between the interests of individuals and groups. Unlike Sorel and Fanon, he does not deem violent conflict necessary. Going beyond Madison and Marx in their assertion of the close relationship between power and property, Rummel notes that it is absurd to suppose that, because the citizenry in socialist countries are by their constitutions collectively vested with most of the property, the citizenry actually control it. He concludes that there is elite control of property in socialist as in capitalist nations and that therefore it is the elite who continue to define justice.

Define *what?* Rummel says that only the individual can freely define justice: when the state does so—as it always has—it imposes its definition of justice and it controls the citizenry. The final solution is "decentralization" (dissolution?) of power from the central government, which is "our enemy." Interim solutions produce transitory, de facto definitions of justice as the product of the balance of powers and of what each individual deems right. Conflict is of course the process by which this momentary, ever-changing sequence of equilibria is established. Rummel does not make clear how the individually different definitions of justice, even freely arrived at, are reconcilable if we do get rid of our enemy, the state; and he does not consider how not only the state but we ourselves are our enemy.

The book is diminished by its lack of the careful cross-checking of definitions and relationships that characterizes texts in physics, chemistry, and biology. Aggression on page 25 is "a disposition, power, or manifestation characterized by assault, attack, invasion." Nevertheless, the author says, aggression does not necessarily involve the desire to hurt others. And there are at least four varieties of aggression: altruistic, instrumental, defensive, and hostile; indeed, there is aggressive defense as well as defensive aggression. And more fundamentally, the book never quite satisfactorily distinguishes between what people want and what people justly want. Rummel does not consider whether people regard their every want as justified or not, and so he may be imposing his definition of everyone's sense of the just on everyone—or letting everyone fight it out with his/her own definition. It is evident that there is some kind of assent between conflicting individuals and groups, as they bargain and fight for what they want and then settle for what they may or may not deem just. *Why* they do this remains unclear, in this book and elsewhere.

If there is thus any fundamental shortcoming in the book, it is the lack of a solid, substantial statement of the process by which new demands are generated within individuals, groups, and nations, and *why*, psychologically considered, these demands generate in the first place and later. In short, there is little of the majestic theory of socioeconomic and political development—from primitive to feudal to capitalistic institutions and beyond—which Marx constructed. And there is little systematic consideration of the wants which are fundamental to all human beings—a basic matter that Marx treated rather casually—and which Freud, James, McDougal, and later students of human motivation have considered deeply.

On page 18 there is a chart, "The Process of Conflict," containing "the conflict helix." It does describe a kind of spiraling process, but the forces that produce the spiral motion are in the book quite formally sketched, with too little use of the psychology that intuitively and very variously was used by Marx, Simmel, Sorel, and Fanon. Rummel's book is nevertheless one of the most provocative attempts at new theorizing by a political scientist. More people should be trying what he has ventured. When they do, they will know the difference between the work of scholars and of critics and will appreciate the rather rough but very large work that Rummel has done.

JAMES CHOWNING DAVIES

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Israel—The Embattled Ally. By Nadav Safran. (Cambridge, Mass.: Harvard University Press, 1978. Pp. ix + 633. $18.50.)

What is the basis for the close "special relationship" generally perceived to exist between the United States and Israel? Does American support spring essentially from emotive sources, such as guilt over the Christian persecution of Jews down the ages, sympathy for the victims of Nazi savagery, identification with a beleaguered democracy and a pioneering, culturally kindred society? Or is it primarily founded on hard-headed conceptions of national strategic interests? What role do domestic political considerations play, given the existence of a large, articulate and politically active Jewish-American community? If all these factors are at work, has the mixture varied over time? How? Why? These questions have been central to any appraisal of U.S. policy in the Middle East since World War II. Following the October 1973 war, they have become fraught with crucial operational import for U.S. policy, as America has become increasingly enmeshed in the Arab-Israeli conflict, while expanding and deepening a multifaceted network of relationships with several key Arab states.

Nadav Safran attempts an answer to these questions through a lengthy, informed and well-written survey of U.S.-Israeli relations from the creation of the Jewish state down to the first months of the Carter administration, and of postwar American diplomacy in the Middle East generally. His analysis, which encompasses relevant aspects of inter-Arab relations and the Soviet role as well, expands and updates his earlier book, *The United States and Israel*, published in 1963.

But this massive volume offers much more than that. Its first 300 pages contain a history of Israel's social, economic and political development, as well as its foreign relations, its unfolding defense-related problems, and its repeated military encounters with the Arab world. Safran's scope throughout is comprehensive, and his command of the subject is sure and thorough. There are occasional lapses (of which perhaps the most serious is the disingenuous treatment of Israel's "liberal occupation policy" in the West Bank and Gaza, on pp. 268–69), and errors (France began major arms sales to Israel in 1954, before the first Soviet-Egyptian arms deal was consummated, not in 1956, as asserted on pp. 353–54). Nonetheless, the first half of the book is a solid survey of Israel's development in its first three decades. It will be useful both for the casual reader and the undergraduate student, though specialists will find in it little that is new by way of data or interpretation.

The core of the work, however, is found in the second half, entitled "Israel and America in International Politics," where Safran laboriously traces the complex interactive evolution of U.S.-Israeli and U.S.-Arab relations against the background of Soviet-Arab ties and U.S.-Soviet clashes over the past three decades. The book elaborates the bases for the emergence of a "moral" commitment to Israel's survival and security in the United States and notes the active role played by U.S. Jews in eliciting this commitment and promoting a supportive climate of opinion. The political weight of pro-Israeli groups and their increasing effectiveness in the domestic arena are acknowledged.

But Safran is mainly concerned with establishing that U.S. support for Israel is founded not merely on a "moral" base, but also on solid, political-strategic grounds, which he insists on labeling as "real" American interests. These are identified as secure access to oil, and Soviet containment, and Israel is portrayed as having gradually come to play a central role as the protector of these American stakes in the area. He notes that the U.S.-Israeli relationship has not been static or unidimensional. While in the economic and cultural spheres the combination of general pro-Israel sympathies and the activism of U.S. Jews have led to a "uniformly and unequivocally friendly and helpful attitude," on the diplomatic-strategic plane there have bene fluctuations. Until the mid-sixties, these fluctuations were caused in the main by the regional requirements of U.S. policies aimed at excluding and later containing Soviet influence in the Middle East. From 1948 to 1957, Washington's efforts to bring the Arab world *into an anticommunist regional defense arrange*ment à la SEATO dictated putting some distance between it and Tel Aviv. The subsequent failure of these schemes, and the entrenchment of Soviet influence in Egypt, Syria and Iraq following the Suez war ushered in a decade of increasing American reliance on Israel's military strength as a counterpoise to the anti-Western nationalist Arab bloc led by Nasser's Egypt.

In the aftermath of the Six-Day War, this perceived conjunction of interests reached a climax, as the Arab defeat brought with it a practically total polarization and a stronger Soviet regional presence. Lyndon Johnson's support for Israel's decision not to evacuate conquered Arab territory until a final peace had been concluded is held to have "marked a crucial turning point in American-Israeli relations in that it wedded America's real interests in the Middle East to Israel's and brought the real American interest into complete harmony with the moral interest in Israel" (p. 583). The United States became Israel's major arms supplier, a role that the next administration fulfilled with relish within the compass of its Nixon Doctrine, and in response to the continuing Soviet arms buildup in Egypt and Syria. The American strategy appeared to pay off handsomely at the time of the Jordanian civil war in the fall of 1970. Washington was able to rely on the threat of unleashing Israeli power in order to protect the Jordanian regime, without a need for direct U.S. military intervention to save Hussein from the neo-Marxist Syrian Ba'th and the PLO. The Jordan events ushered in a new period, which was to last until the October 1973 war, marked by "unequivocal military support for Israel and increasing identification with its policy conceptions" in order to frustrate Soviet designs and protect the oil-rich Arab countries (p. 587).

At the descriptive level, Safran's analysis is by and large unexceptionable. Historically, there is no doubt that, from the days of Truman and Acheson through the Nixon/Ford-Kissinger era, Washington struggled through the thicket of Middle East politics while its eyes were compulsively and unwaveringly fastened on Moscow. As the 1950s and 1960s wore on, Israel indeed came to be regarded as the most reliable anti-Soviet force in the area. At the analytical level, however, there is one basic shortcoming. Safran fails to address an issue central both to a clear understanding of the subject and to the persuasiveness of his own thesis regarding the congruity between the U.S. "special relationship" with Israel and America's strategic interests in the Middle East: viz., the extent to which the "special relationship" itself was *responsible* for originally bringing the Soviets into the Middle East, and subsequently fomenting conditions favorable to the expansion and entrenchment of Moscow's presence in the Arab states. In fact, the book's weakest chapters are those that deal with the crucial formative years 1948–1956. One would like to know what role U.S. and general Western support for Israel in the late 1940s and 1950s did play in antagonizing Arab nationalism and forestalling the growth of amicable relations with the United States. To take one specific example, on the vital issue of arms supplies, how did the constraints on U.S. sale of weapons to Egypt and Syria imposed by the requirements of the commitment to Israel's security affect the calculus of decision that led Cairo to Peking and Moscow in the mid-1950s?

Once the Russians were ensconced in the heart of the Middle East, there is no doubt that, within a cold war context, a strongly anti-Soviet Israel came to serve useful American strategic purposes in the area. But, unless one is to argue that Moscow's penetration was somehow an inevitable process (which Safran does not do), any thorough analysis must look at the genesis of the Arab involvement with the Soviets and evaluate the impact on this process of the early and close American association with Israel's emergence and growth. At the time, it should be noted, this association was the product of the combined effect of "moral" U.S. concerns and the pressures of American Zionists and their supporters on the Truman administration. Those who argued from a "strategic interests" perspective then were ranged in opposition to that policy.

Because it is incomplete, Safran's argument remains unconvincing. One can easily stand his analysis on its head and argue that U.S. policy, having failed to prevent Soviet inroads into the Middle East largely because of its association with Israel, came to rely on the Jewish State as its principal anti-Soviet ally willy-nilly, since alternative policies (pro-Arab) were not available to Washington as they were foreclosed by the realities of domestic politics.

Similarly, after the June war, during Safran's "Golden Era" in U.S.-Israeli relations, American policy was based on a major misconception that would become painfully obvious in October 1973. Washington assumed that a strong Israel in the context of a regional military balance of power would deprive the Arabs of a credible military option and force them into a peace settlement on Israel's terms. The 1973 war and accompanying oil embargo invalidated these assumptions and created a totally new set of circumstances. Further encouraged by Sadat's rift with Moscow, U.S. diplomacy became geared to active promotion of an overall peace settlement and rapprochement with the Arabs. This had led to recurrent crises in U.S.-Israeli relations. Yet, Safran argues, paradoxically the basic interests of the two countries remain interdependent. It is America's continuing strong support for Israel and Israel's continuing control over Arab territories that give the "moderate" Arabs the required incentives to accept U.S. overtures and seek America's friendship (pp. 588–89). Hence, the author concludes, the United States should in the future preserve the "special relationship," since "that relationship is important both in itself and in order to make it desirable for the Arab countries to continue to seek the goodwill of the United States." This will require a mutual security treaty between Israel and the United States similar to the American-Japanese treaty. Armed with such guarantees, Israel will be able to make territorial concessions necessary for an Arab-Israeli settlement (p. 598).

Here, again, Safran unexplainably postulates a natural incompatibility between Arab and Western interests, requiring the corrective agency of a continued Israeli-U.S. alliance. That friendly relations between the United States and important Arab countries are incapable of developing their own self-sustaining basis is—from the "national interests" perspective as defined by Safran himself—certainly not self-evident. A mutual defense treaty with Israel probably will be a necessary part of the American contribution to an Arab-Israeli peace. But it will be the price to be paid for domestic support of an active U.S. role in bringing about a settlement acceptable to the Arabs, not the expression of a reasoned, historically valid calculus of American strategic interests in the Middle East.

PAUL JABBER

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The Politics and Economics of European Monetary Integration. By Loukas Tsoukalis. (Reading, Mass.: Allen and Unwin, 1977. Pp. 192. $18.75.)

Loukas Tsoukalis has recently entered the field of finance through the academic gate. Being a "double-header," i.e., well qualified in economics and politics, he regrets that "the apparent technicality of international monetary issues has discouraged political scientists from dealing with the subject" (pp. 11–12), and also that "the complicated terminology which has developed in this field has created almost a closed shop for specialists" (p. 12). Both assessments appear accurate. In addition one will have to agree with the author that money has become "highly political" (p. 171).

However, it was the communication problem that may have motivated Tsoukalis to translate his *opus magnum* (which had just been submitted to Oxford University) into a modified version that would presumably be accessible to a larger audience.

While working on his doctoral dissertation, Tsoukalis faced monumental hurdles. One of them was the fact that, as he puts it in the preface, "International monetary affairs have been traditionally covered by a veil of mystery and secrecy." To overcome that obstacle Tsoukalis engaged in numerous interviews with people in the decision-making process in the various EEC countries, and in consulting press cuttings as essential sources of information. Such a research strategy known as "communications analysis" is methodologically far from flawless since it lends a speculative quality to scholarly analysis. The reader is left with many questions, the main one being: "Why should decision-making intimate in a private interview what they refused to make public?" Fortunately, however, the present work draws on a large range of written sources as well. In fact, the interviews have not been listed, and one is consequently left in the dark as to who among that "large number of Community officials and members of national permanent delegations" (preface) has been consulted, and on what subjects. Nor did the author see fit to add a bibliography.

Tsoukalis' approach is strongly theory-oriented. Indeed, an entire introductory section is devoted to adding a theoretical background. The first chapter in particular tends to render the argument abstract. Although not all of it is original, one is forced to admit that Tsoukalis offers some fresh perspectives in his explanation of the various schools. Regrettably, he has excluded the communications school from his consideration. Although that omission is justified in a footnote (p. 177), one is left with the impression that maybe in this instance Deutsch et al. miscommunicated. Also, one is struck by the ambiguity of purpose: the author wants his analysis to rely on as well as to test existing theories.

The other theory chapter is concerned with optimum currency areas and the EC. Peter Coffey in his recent book *Europe and Money* pointed out that "an optimum currency area is a different animal from an economic and monetary union." Nevertheless, Tsoukalis' discussion which includes a number of important definitions is not only intriguing but also gradually gaining in relevance. It is then that the author arrives at the *pièce de résistance,* the actual object of his study, EMU (there is a somewhat ominous irony in the fact that the initials of Economic and Monetary Union also refer to a "flightless and ostrichlike bird!"). Defining the limits of his inquiry as "the history of EMU in the European Community, the policies of the parties involved and the conflicts of interest created in the political and economic environment within which all this has taken place," the author concentrates on a period of five years starting in 1969. Historically, this may well be one of the most significant episodes in the development of the Community, if not a fateful turning point. It marked the addition of three new members, the acceptance of the all-important Werner and Tindemans Reports, and the birth of that other animal, the snake. In a sense one may say that the alternatives inherent in the dilemma *élargissement ou profondissement* were simultaneously pursued. The snake, one will remember, has met disaster upon disaster. Having left the tunnel it was variously afflicted by defections, irregular membership as well as eventual dismemberment. All that is left now is a mini-snake which is little more than a Deutsche mark satellite zone. That too is bound to disappear if the resolutions adopted at the Bremen and Aachen summits are executed. However, what is so surprising of Tsoukalis' work is not that so much has become dated (which is what one would expect of the kaleidoscopic mutations in the world of finance) but that a great deal is still relevant. Indeed, as an historical analysis the book will retain considerable relevance until EMU has definitely emerged, that is, until the process of monetary integration has been completed.

The study is without question based upon painstaking research. Its style, though fairly flat at times, is at least lucid, lacking the involved prose that has become endemic in academic writing. And most importantly, Tsoukalis has also succeeded in building a bridge between politics and economics by rendering complex matters comprehensible to students in both disciplines.

HENRI J. WARMENHOVEN

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Reducing Global Inequities. By W. Howard Wriggins and Gunnar Adler-Karlsson. (New York: McGraw-Hill, 1978. Pp. xiv + 193. $9.95, cloth; $5.95, paper.)

This is another volume in the 1980s Project of the Council on Foreign Relations. It contains two loosely connected studies by Howard Wriggins, currently Ambassador to Sri Lanka, and Gunnar Adler-Karlsson of Denmark.

Wriggins' study focuses on Third World strategies for change—essentially external strategies that the developing countries have adopted and might adopt in the future to gain some negotiating leverage against the developed countries. Six strategies are discussed: commodity cartels, regional coalitions, universal coalitions containing the great majority of Third World countries, associations with a major power, the threat or precipitation of local war, and irregular violence. In each case Wriggins briefly outlines the advantages and disadvantages of a strategy and the conditions that have favored or may favor its adoption, and then concludes with some speculations about its future attractiveness. Very brief case studies accompany each discussion.

Wriggins concludes, not surprisingly, that Third World states are not as helpless as many of their leaders contend, that recent events have given these states wider choices, and that all of the strategies, while likely to remain in use, have significant limitations, especially given the real or potential conflicts of interest within the Third World.

A catalogue of past strategies by its nature cannot break much new ground. Still, within the constraints set by the analytical framework, Wriggins' comments seem generally sound. One might argue with details in some places, but the author is usually careful to qualify his arguments and to indicate the limitations of a list of strategies as a guide for the future. The result, nevertheless, is a useful essay, perhaps especially as background for students first encountering the problems of the Third World and the international system.

Adler-Karlsson's study concentrates on the need to reorient development policy toward the direct reduction of absolute poverty. He sets out the by-now familiar statistics on the vast number of people in the Third World who are deprived of adequate food, water, health, housing, and other basic needs. And he argues, as many have before him, that the first concern of development must be to meet these basic needs, that conventional growth theory and its "trickle down" mechanism have failed in this regard, and that successful implementation of a basic needs strategy will require a direct assault on domestic structures of inequality in the Third World and on the inequalities built into the international trade and aid system. He contends also that it is not scarcity of material resources—a fear of some who have argued (mistakenly, I believe) that an emphasis on direct relief of poverty will demand vast and unlikely resource transfers—but the lack of political "will" domestically and internationally that impedes progress.

The weakness in Adler-Karlsson's argument is not really in what he says, but rather in what he does not say. Support for a basic needs strategy is now widely shared—the World Bank under Robert McNamara, the Agency for International Development, the ILO, and various intellectual and public figures in both the developed and developing world have been advocating such a change in development strategy for some years. But there are both conceptual and political problems implicit in this change, and Adler-Karlsson fails to deal with any of these problems. As a result, we have advocacy and prescription, but not analysis. And surely the latter is what is now necessary, unless we want to take the risk of stumbling into another round of chasing the latest fashion in development circles and perhaps generating another set of unforeseen consequences.

In large part what is absent from Adler-Karlsson's treatment is analysis of the very difficult "how" questions: for example, how to convince Third World governments that basic needs is not an anti-growth strategy consigning them forever to extreme dependency and how to conceptualize the relationship between the reduction of poverty and inequality (two very different notions) and the maintenance of sufficient growth. Without analysis of these and other such questions, his study is useful only as exhortation.

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The Missile Defense Controversy: Strategy, Technology, and Politics, 1955–1972. By Ernest J. Yanarella. (Lexington: The University Press of Kentucky, 1977. Pp. xi + 236. $17.25.)

The main themes of this book can be summarized as follows:

An American "technological imperative" (according to Ralph Lapp, "if a weapons system *could* be made, then it *would* be made"—p. 4) has been the "real" stimulus of military technology and the strategic arms race of the 1960s. Rather than being an objective, deterministic force, the technological imperative has "subterranean" sources in human relations—specifically the U.S. military research and development organization. In this centralized and closed R&D system, an internal arms race has been run since the beginning of Secretary of Defense McNamara's administration, pitting the dominant offensive, strategic systems against the subordinate defensive systems, "with little or no regard for any real Soviet threat or developing Soviet capability" (p. 100). Individual responsibility falls mainly on Secretary McNamara, whose strategic and managerial approaches not only predominated in his own administration but also conditioned future debate and decisions up to and beyond the "unwitting" Nixon Administration that agreed in SALT I to freeze ABM development. The U.S. had "overwhelmingly" great latitude for slowing the arms race, given its innovative role in arms technology (and the USSR's imitative role and acceptance of technological inferiority), but the opportunities for greater restraint in offensive systems and for pursuit of defensive systems were neglected by McNamara due to the technological imperative and to his bias for deterrence based on offensive systems.

The foregoing summary does not of course allow for some of the qualifications in the book. Indeed while the author repeatedly emphasized the predominant role of the technological imperative (e.g., pp. 99, 165, 194), he also suggested the contrary in the preface (technology does not limit the function of politics—p. ix), acknowledged evidence to the contrary throughout the book, and "conceded" in the conclusion that many factors spur the arms race even though the book "argued that bringing military R&D (especially in the offensive realm) under effective control would be tantamount to controlling the arms race" (p. 202).

An alternative summary could therefore conclude that the ABM case refutes the technological-imperative thesis—i.e., that political and strategic factors were the primary reasons why deployment of the ABM was postponed for many years, why deployment was finally approved, and why ABM deployment was stopped under SALT I. These contradictory interpretations suggest at the least that the author did not entirely achieve one of his stated objectives: weighing rather than simply delineating the impact of strategic, technological, political, organizational, and economic factors.

The themes outlined in the first summary would have been more convincing if the following issues, among others, had been clarified.

The characterization of McNamara's strategy of assured destruction as simply a return to the previous administration's strategic doctrine ignores, among other distinctions, the qualitative improvements (due to technological innovation in missile and command and control systems) which contributed to a more stable strategic balance in the 1960s.

The criticism that McNamara's cost-effectiveness approach fueled the arms race does not explain the decline in U.S. strategic arms budgets from the mid-1960s to the mid-1970s, nor does it deal satisfactorily with the judgments of analysts such as Klaus Knorr and Oscar Morgenstern that the "conservative" McNamara techniques tended to discourage technological innovation. Moreover, Yanarella mentions McNamara's cancellation of several important weapons systems but does not explain how this fact squares with the repeated assertions about the predominance of the technological imperative.

Yanarella is justified in criticizing a simple action-reaction explanation for the arms race and the use of "worst-case" estimates. But he neglected to deal with the arguments of analysts such as Albert Wohlstetter (in 1969, 1970, 1974, 1975) to the effect that in many instances the U.S. underestimated the capabilities of Soviet weapons development. Moreover, the concepts of an "internal" arms race within the U.S. and American primacy in causing the arms race undervalued the significance of American perceptions of threat, risk, and uncertainty in the international system and the role of the USSR. Even the multiple-factor causality is "conceded" in the book, the factors singled out seem to be directed only to the U.S.: "the role of the tradition of 'negotiation from strength,' the pervasiveness of bureaucratic politicking, and the 'imperatives' of capitalist expansionism and planning for obsolence" (p. 202). The steady Soviet buildup in armaments since the late 1960s—including a number of new strategic weapons systems—suggests that the USSR is not willing to accept technological inferiority and that their increased technological sophistication may be promoting a technological "breakthrough" approach that Yanarella ascribes only to the U.S.

Although the author questions the conventional wisdom that emphasizes the preeminence of the strategic offensive in the nuclear era, he offers no unconventional wisdom about the alternative of relying on defensive systems. He simply calls for "transcending" the mutual assured destruction strategy and for a "radical" and more "holistic" approach to disarmament by the U.S. McNamara believed that stopping the ABM was a means to dampen the arms race and to preserve deterrence stability. This book argues that the USSR, following in their defensive tradition, would have responded to an American ABM with more of their own defensive systems and not, as McNamara maintained, with more offensive weapons. The book does not explain why the USSR agreed to an ABM halt under SALT I. If one reason was to stop a more sophisticated American ABM technology, then it seems more likely that Russia would have responded to an American ABM not with their less sophisticated ABM system but with what they seem to do best—i.e., producing more offensive missiles.

Since this case study has implications for the contemporary strategic arms competition, especially in light of the growing vulnerability of land-based ICBMs and some renewed interest in ABM, it is unfortunate that the book was published (December 1977) about three years after the writing evidently had been completed. Although the book claims to have uncovered the "clandestine" and "underlying roots" of the arms race, a more successful effort to explain the interrelationships of technology, politics, and strategy as manifested in the U.S.-USSR arms competition would have required analysis of issues that are only touched upon in this volume. Admittedly an expanded exploration of the problem would have lengthened the book considerably beyond its 204 pages of text. But that would have been a better book and $17.25 a fair price.

EUGENE J. ROSI

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